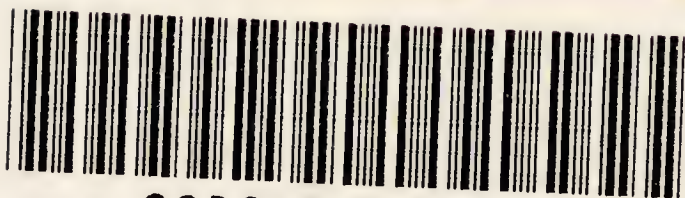



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STUDIES ON MALARIA

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FABLES.

PLATE I



WILLIAM CRAWFORD GORGAS, RONALD ROSS, HENRY CLAYE WEEKS,
On board the S.S. "Advance" at New York on September 27, 1904

Frontispiece

845-10 B 616.433

STUDIES ON MALARIA

BY

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LONDON

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1928

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INSCRIBED TO THE MEMORY OF
SIR EDWIN DURNING-LAWRENCE, BART.,
AND TO THE NAME OF
EDITH LADY DURNING-LAWRENCE,
WITH THE THANKS OF THE AUTHOR
FOR MUCH HELP
FREQUENTLY GIVEN.

PREFACE

THE formal history of my own researches on malaria is contained (1) in the annals de l'institut Pasteur, 1899 ; (2) in my Lecture for the Nobel Medical Prize for 1902 (reprinted Journ. Roy. Army Med. Corps, April, May, June, 1905) * ; (3) in my Prevention of Malaria (Murray, 1911), and (4) in my Memoirs (Murray, 1923). But as the first of these is in French, the second in three consecutive numbers, while the third (out of print) and fourth are comparatively large and expensive volumes, I have no small and convenient book on the subject which I can recommend my numerous correspondents who are interested in the subject, to purchase. I hope that this work will satisfy them.

My thanks are due to Miss Maude Lafford, of the Malaria Department of this Institute, for much help rendered in the preparation of this book.

RONALD ROSS.

10th July 1928.

* German translation by C. Schilling (G. Fischer, Jena, 1905). Italian translation by F. Maiocco (Lib. Editrice Universitaria, Torino, 1905). French translation by Ch. Broquet (N. Maloine, 27, Rue de l'Ecole de Médecine, Paris, 1928).

CONTENTS

	PAGE
PREFACE	vii
CHAPTER I PREVIOUS DISCOVERIES AND SPECULATIONS	I
„ II FIRST STAGE OF THE INVESTIGATION .	4
„ III THE SECOND STAGE, 1897-1898 . .	14
„ IV THE THIRD STAGE, ITALY, 1898 . .	19
„ V THE THIRD STAGE, FREETOWN, 1899 .	41
„ VI THE FOURTH STAGE, FREETOWN, 1899 .	47
„ VII WAITING, 1899	56
„ VIII STILL WAITING, 1900	60
„ IX AN AMERICAN DISCOVERY, 1900 . .	70
„ X A DASH FOR VICTORY, 1901	74
„ XI SIR WILLIAM MACGREGOR, 1901 . .	87
„ XII FREETOWN, 1901-2	102
„ XIII ISMAILIA, 1902	113
„ XIV PANAMA, 1904	121
„ XV GREECE, 1906	129
„ XVI MAURITIUS, 1907-8	134

CONTENTS

	PAGE
CHAPTER XVII INDIA, 1909	137
„ XVIII SPAIN, CYPRUS, GREECE, 1913	140
„ XIX THE WAR AND AFTER	144
„ XX STUDIES ON PATHOMETRY	154
„ XXI SUMMARY OF FACTS ABOUT MALARIA	160
REFERENCES	179
INDEX	191

LIST OF ILLUSTRATIONS

		FACING PAGE
I	W. C. Gorgas, Ronald Ross, H. Claye Weeks on board the S.S. <i>Advance</i> at New York on 27th September, 1904	<i>Frontispiece</i>
II	Charles Louis Alphonse Laveran	2
III	Patrick Manson	16
IV	Malcolm Watson	151

NOTE

The figures in square brackets throughout the text apply to the References (pp. 179-189).

CHAPTER I

PREVIOUS DISCOVERIES AND SPECULATIONS

THIS book is concerned chiefly with my own work, and I should therefore perhaps begin by mentioning that I was born at Almora in the Himalayas in 1857 ; was sent to England in 1865 ; went to school there ; entered St. Bartholomew's Hospital for the medical profession in 1875, without any predilection on my own part ; joined the Indian Medical Service, and was appointed to its Madras Branch in 1881. During the first seven years of my service I employed my leisure chiefly on the study of mathematics and literature ; but became gradually convinced that the most profitable manner in which a man can occupy himself (for others) is to investigate the great infectious diseases which destroy or maim so many millions of people ; and I expressly selected malaria as a subject which was not then well understood but which caused an enormous amount of sickness in almost all warm countries.

At that time everyone thought that malarial fever is produced by some kind of poisonous exhalation from damp areas such as marshes (not so wrongly, after all). My first studies led me rightly to doubt altogether the hypothesis of an *aërial* miasm, because

the matter [11, 13]. Scarcely anyone had taken up the subject in India except Vandyk Carter, who had confirmed Laveran's work but did not mention Golgi, so far as I remember. When I commenced work I was Staff Surgeon at Bangalore ; but when I returned to England on furlough in 1894, Dr. Patrick Manson showed me specimens of Laveran's crescents and flagellated bodies—stages of the malaria parasites—which convinced us both that Laveran's bodies were genuine parasites, though we could not explain what we had seen. The late Dr. H. G. Plimmer had previously demonstrated these bodies to Manson. In the autumn of 1894, Manson told me his hypothesis that the flagellate bodies are stages of Laveran's parasites which produce motile spores for the purpose of infecting mosquitoes which happen to suck the blood of patients, and he published a note on the subject immediately afterwards [14].

I accepted Manson's views tentatively pending further researches, which I could not begin until 1895, at Secunderabad in India. I had of course commenced the study of malaria years before I ever met Manson.

CHAPTER II

THE FIRST STAGE OF THE INVESTIGATION

THAT was by no means a propitious time (13th May 1895) to begin a difficult investigation on malaria in India, and I was only an Army doctor, who found it very difficult to obtain instruction or advice on any medical subject, or even the necessary books. I worked almost solely by myself in my little regimental hospital at Secunderabad. Almost nothing was known about Indian mosquitoes and I was much laughed at for my pains. Not only so, but my colleagues seemed mostly to think that I was a kind of charlatan. Almost by every mail (once a week) I wrote home to Manson describing my progress and its results, and he advised and encouraged me in return. I used to keep mosquito-larvæ of various kinds (which I found round the barracks) in bottles, until they reached maturity, and then to feed the insects on patients who possessed crescents in the blood, giving these patients one or two annas for every mosquito which had sucked their blood. For this purpose the patients were at first made to lie down naked inside a mosquito-bed-net into which the adult insects were liberated until fed, being then collected by an assistant in test-tubes ; but later I found a better way, of applying a

test-tube containing a single living mosquito directly to the patient's skin. The insects were then killed with a puff of tobacco smoke or a drop of chloroform, and were dissected at once ; or were kept alive for future dissection.

At first I studied more carefully the transformation of crescents into flagellate bodies, described by Laveran and others, and found that this transformation occurs to about 40 *per cent.* of the crescents sucked up by a mosquito, but only to a smaller percentage of them when extracted in a droplet of blood from the finger [17]. This was very encouraging, because it suggested that mosquito-drawn blood, and not finger-drawn blood, is the proper place for the transformation ; but I failed in finding any further development in the so-called flagellated spores, as Manson expected, because in fact these bodies are not spores at all, as Manson had thought, but are sexual bodies—microgametes—as discovered by MacCallum two years later [28]. During the first few months of my work I also found the best method of dissecting mosquitoes, how to distinguish *Culex* from *Aedes* mosquitoes ; a gregarine of the latter, which I fancied might be the malaria-parasite in it, and other details ; but I had scarcely got fully to work when I was ordered off for special sanitary duty at Bangalore (9th Sept. 1895).

For six months I was fully occupied with this new duty at Bangalore, trying to ameliorate the health-affairs of that station, but continued the malaria-work when possible. Later I tried to produce malaria in healthy persons by the bites of *Culex* or *Aedes* which

6 FIRST STAGE OF THE INVESTIGATION

had been previously fed on malarial patients, but without success [23].

In March 1896 Manson delivered three Goulstonian Lectures on the life-history of the malaria-parasites outside the human body [20]. In the last of these lectures he gave a detailed account of my recent work. I thought that his original idea about the flagellate bodies being meant to infect mosquitoes (though quite right) was over-elaborated in these lectures, as he went on to speculations which were not justified by the existence of the flagellate bodies, such as an hypothesis that the *Plasmodia* are really parasites of mosquitoes independent of men, infecting men through water or inhaled dust.

The Goulstonian Lectures, however, roused the opposition of Dr. A. Bignami (a Roman physician). Some time previously (I cannot ascertain exactly when) a hypothesis that the flagellate bodies are not vital stages of the malaria-parasite at all, but are produced merely by the dying struggles of these little creatures on the microscope-slide, had gained a footing in Italy and elsewhere. Apparently also, the hypothesis had been started by G. B. Grassi ; but no one who had really studied these bodies could believe this tale, much less myself who had studied hundreds of flagellate bodies in finger-blood as well as in blood sucked up by mosquitoes. On one occasion I had seen numbers of the flagellated spores (as we then thought them to be) issuing from the parent cell, just as you might imagine a number of snakes issuing from a bag at various points ; and on another occasion had watched one wriggling about by itself for hours.

In fact the process seemed to us to be clearly a vital process and not one of death or disintegration ; but the influence of Dr. Bignami was so great that many observers adopted this hypothesis of his and of others. His paper [21] appeared in Italy on the 15th July 1896. Manson sent me a translation which appeared later in the *Lancet* (14th and 21st November 1896). It was clearly necessary for someone to prove definitely that the flagellate bodies are not due to dying struggles ; and I found a simple method by which such proof was obtained, and published a paper upon it [25].

As everyone knew, the flagellate bodies do not appear at once after the blood is drawn but some minutes later, and then only in a percentage of the mature crescents. I placed a small mass of vaseline on a patient's finger, pricked the finger through the vaseline, squeezed out a drop of blood into the vaseline, and then scraped off the whole mass, still enclosing the droplet of blood, by the edge of a coverglass. The mass was then flattened out by pressing the coverglass on to a glass slide and could be examined at any time subsequently. The blood was thus not exposed at all to the air and the consequence was that none of the crescents became flagellate bodies ; though this happened regularly enough if the specimen was opened and exposed to the air an hour or two later. After 24 hours, however, the crescents became disintegrated and showed signs of death, but without becoming the flagellate bodies. Though all the crescents die in the specimen, none of them exhibit the forms which Bignami claimed were dying struggles. It seemed to me that this gave a complete disproof of the dying-

8 FIRST STAGE OF THE INVESTIGATION

struggle-hypothesis. Manson and Rees repeated my experiments and confirmed them later. Next year it was shown by W. G. MacCallum in America that the so-called flagellated spores are really not spores at all, but microgametes [23]. My experiments showed that *abstraction* of water by drying or by the mosquito's stomach was required to produce flagellate bodies, but Marshall in the Lancet, 24th October 1896, had shown that ex-flagellation is also expedited by *adding* water to the blood.

In spite of my disproof, Bignami (and Grassi) still maintained their dying-struggle-hypothesis, in the long paper mentioned [21], which was something like that of King [9], and suggested that malaria was like piroplasmosis, which had been shown by Smith and Kilborne [12] to be carried by ticks, though not by a tick which has fed on an infected ox but by its progeny. I doubted that there was any basis for this hypothesis; and, later, in 1898, failed in finding any malaria-parasites in the eggs of infected mosquitoes [37]. This paper of Bignami was quite proper though not sound. I may add that N. Sakharoff [15] had already shown that the so-called flagella did contain chromatin in spite of Bignami's belief to the contrary, though Bignami was not aware of the fact in 1896; while, of course, the papers by myself as mentioned above [25], and by MacCallum [28] finally disproved his dying-struggle-story.

Early in 1897 my special duty at Bangalore ended, and I took my wife and family to Ootacamund on leave and continued my malaria-investigations round that place. There I found various other parasites

in mosquitoes and discovered another type of mosquito, with spotted wings, in the Sigur Ghat close by, and clarified my ideas with thought and experience. At the end of my leave I returned to another regiment at Secunderabad. Here, on the 20th and 21st August 1897, I discovered in the stomach-tissue of two spotted-winged mosquitoes, probably *Anopheles stephensi*, previously bred from the larva in bottles and then fed on a case containing numerous malaria-crescents, certain cells which possessed pigment-granules precisely similar to the pigment-granules of the malaria-parasites.

Although of course I could not yet be quite certain that those cells were actually the malaria-parasite living in the spotted-winged mosquitoes, I had reason to suppose so and wrote to tell Manson what I had found. A month later I obtained the cells again in another species of *Anopheles* (probably *A. culicifacies*), fed on malaria blood and bred from the larva in my laboratory. The same or next day I was suddenly and inexplicably ordered away at once to another station (Kherwara, Rajputana), a thousand miles away, on duty (p. 13).

But I had really solved the malaria-problem during those two years' hard but negative work in India. The cells which I had found proved to be, as I anticipated they were, the malaria-parasites growing in the spotted-winged mosquitoes. I now knew the appearance of the cells in the mosquitoes, the part of their bodies in which they were to be found, and also the probable kind of mosquitoes concerned with that species of parasite. It was the end of the

10 FIRST STAGE OF THE INVESTIGATION

first stage of the research, which had lasted from the 13th May 1895 to the 20th August 1897. Originally there was nothing to indicate any of the three unknown quantities which I had so fortunately found on the latter date. Many discoveries are made merely by chance, but mine had been expressly sought for and achieved under many difficulties by hard work ; and all that has subsequently been done on the subject has been done in the light of that lucky observation of the 20th August 1897.

During all this time I had worked entirely at my own expense, both of time and of money. It was most singular that though malaria caused about one third the total number of admissions into hospital throughout India on the average and therefore a large part of all medical expenses there, and though there were then about a thousand British Government doctors in the country, yet scarcely one of them had endeavoured to investigate this subject, except Vandyke Carter and C. H. Murray (of whom I did not hear until later—Scientific Memoirs by Officers of the Medical and Sanitary Departments of the Government of India, 1887, 1897). Laveran's discovery was not even generally used for clinical diagnosis, for which it is now essential, until much later. Even such keen workers as my friend Patrick Hehir, like myself and others, had been completely misled at first as to the malaria-parasites ; and it was not until the Schools of Tropical Medicine were suggested by Manson at the beginning of the present century that medical men generally began to know better. An enormous loss of life must have occurred between 1880

and 1900 in India alone owing to these circumstances, as all sorts of cases were frequently treated with quinine quite indiscriminately and were seldom treated long enough to be really cured of malaria, when they did have it. No one seemed to see the point of Manson's induction and my own labours were only laughed at.

Before I left England in 1895, Manson had introduced me to the important volume CL of the New Sydenham Society's publications, containing translations of the valuable books by Marchiafava and Bignami, and by Mannaberg on the parasites of malaria; and to several other works, from all of which I profited much during my solitary labours, but scarcely any other malaria-literature except the medical journals reached me at that time; and I had to find out almost everything regarding mosquitoes for myself—their classes, their habits, and the best method of dissecting them by pulling out their alimentary canal. Manson, who had investigated only such large parasites of mosquitoes as *Filariae*, had apparently contented himself with doing little more than flattening dead mosquitoes under coverglasses and was not acquainted with my method of dissection (Memoirs, p. 189); I was obliged to search every micromillimetre of my insects under a strong oil-immersion lens for bodies scarcely larger than red blood-corpuscles and, moreover, to examine every part of their tissues for those hypothetical bodies which I was seeking.

It is curious that even my rough classification of mosquitoes in 1897 into the three chief groups which I then called grey mosquitoes, brindled mosquitoes,

and dappled-winged mosquitoes holds good to-day under the names *Culex*, *Aedes* and *Anopheles*. I fed my mosquitoes by releasing them out of bottles in which I had bred them from larvæ into mosquito-nets in which my subjects of experiment lay naked—as already stated.

That “ex-flagellation” is meant by nature to infect mosquitoes was a profound induction of Manson to which I always give the highest praise; but it did not lead me very far. It gave me no clue as to the species of mosquito concerned. Only one genus of mosquitoes carries human malaria; only some species even of that genus do so; and only a proportion of individuals, even when they belong to a right species, actually became infected by sucking infected blood. Moreover, Manson’s induction gave no clue as to which part of the mosquito’s body was likely to be occupied by the malaria-parasites—which are scarcely as large in comparison with a mosquito as half a crown is in comparison with a hippopotamus. It did not indicate even the form or appearance which the parasite takes in the insect. In short, I had to discover something of which I did not know either the site or the appearance in an animal of which I did not know even the species. All the facts had to be discovered simply by the methodological system of trial and error—that is, by incessant work. It was a wonder that I succeeded at all, even after two years. A distinguished investigator stated a little while ago that it is often difficult to find a single infected mosquito even now when the species of mosquito and the form and position of the parasite in it are well known.

I had to do this before any of these data were given. The twentieth of August ought to be observed as a day of rejoicing, in India at least, by the public.

I reported my discovery of the "pigmented cells" to my Chief in the Madras Presidency, but this did not prevent my receiving urgent orders a few days later to go from Secunderabad to Kherwara in Rajputana. No one, I suppose, believed that I had really discovered anything of any genuine importance. I had told Manson that he might expect to learn the whole life-history of malaria in mosquitoes in a few weeks; but my plans were now quite disorganized. At Kherwara, the cold dry weather was now commencing, and I was scarcely able to do any more mosquito-work at all until next spring (1898); this was a pity because, just before my orders to go away arrived at Secunderabad, I had found the "pigmented cells" again, this time in another species of "dappled-winged mosquito" (*A. culicifacies*), as I have said, which abounded round the barracks that year, and I possessed trained assistants, numerous cases, and everything necessary for my researches.

CHAPTER III

THE SECOND STAGE, 1897-1898

ON the 29th January 1898, however, I received the welcome news at Kherwara that I was shortly to be placed on special duty for six months under the Director-General, evidently to continue my malaria-researches, and I arrived in Calcutta for this purpose on the 17th February. I owe this to the kind influence of Manson who was then practising as a specialist on tropical diseases in London, and who also informed me about the same time that MacCallum and Opie had proved in America [28] that the crescents are not special sporulating forms as he had thought, but sexual forms of which the females evidently develop into my "pigmented cells." Of course I was working not primarily to improve academic science, but to find exactly how malaria is communicated to man. It was much the same thing to my purpose whether crescents are sporulating forms or sexual forms.

After I had obtained the clue on the 20th of the previous August, it was now my task to work out step by step, by the unfailing methods of the microscope, the exact development of the malaria-parasites in mosquitoes and to establish the real nature of

my "pigmented cells"—a comparatively easy labour. But, owing to several difficulties, such as the scarcity of human cases of malaria in Calcutta and the Haffkine-plague-scare which caused many patients to refuse to allow me even to prick their fingers, I was soon forced to work with birds' malaria instead of men's malaria, rightly expecting of course that both these kinds of parasite would have the same, or similar, developments in mosquitoes though not necessarily in mosquitoes of the same species. It was not until the 10th March that I found the exact species of mosquito capable of carrying the birds' malaria (*Plasmodium relictum**) by feeding a number of mosquitoes of different kinds on infected birds and then observing which kind of mosquito became infected from the birds. It was merely *Culex fatigans*, a common species which carried this avian parasite; and within a few weeks I was able to trace the development step by step in these mosquitoes. After a short subsequent check, and a delay caused by the necessity of writing a full descriptive report of my work for the Director-General in May, I was finally able to prove that the "pigmented cells" grow to a considerable size on the outer wall of the mosquito's stomach and there produce spores after about a week.

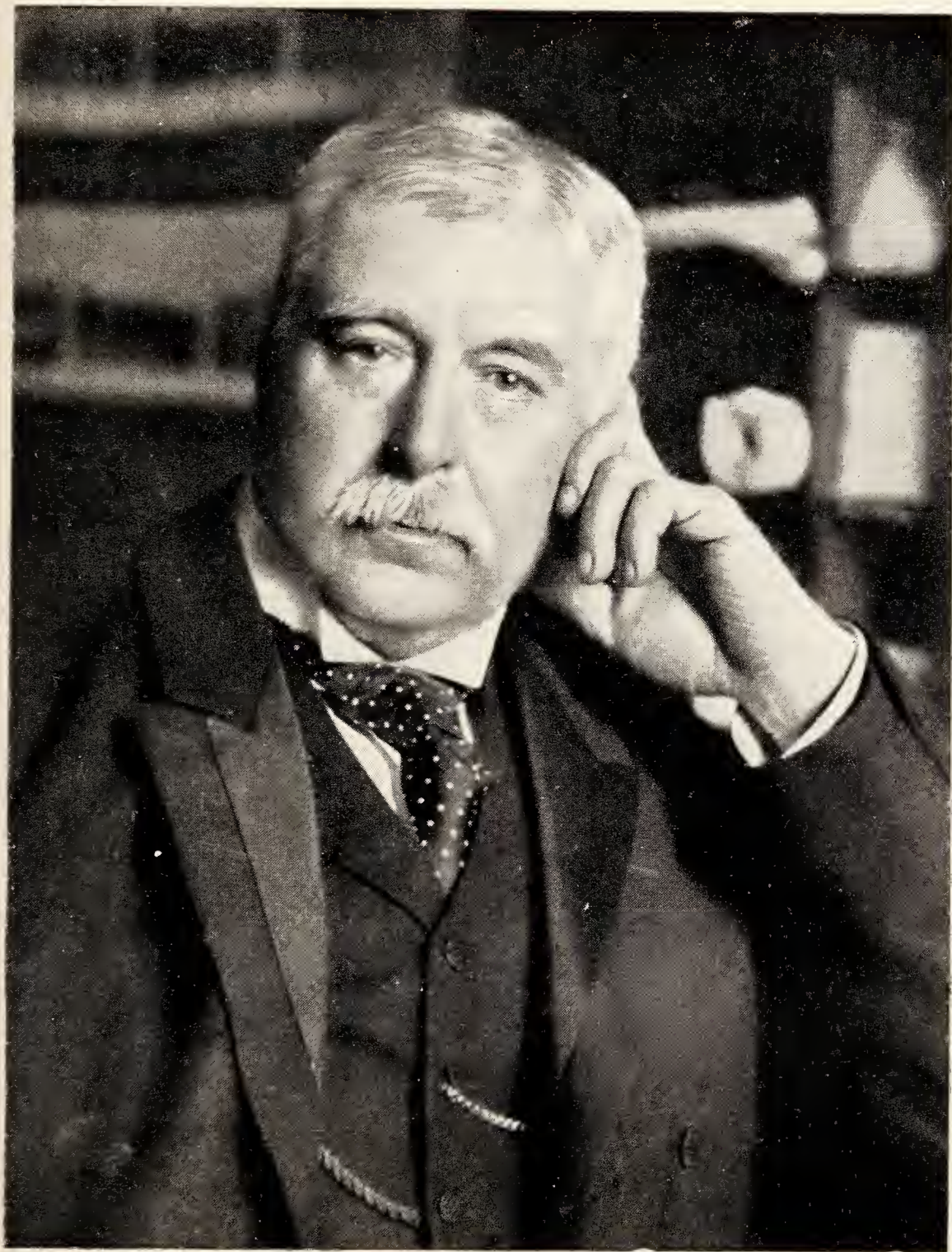
What becomes of these spores neither Manson nor I had imagined, until I found ultimately that they work their way into the mosquito's salivary glands, evidently in order to infect healthy birds when an infected mosquito feeds again upon the latter. This cleared up nearly the whole story, and showed the

* vel *Proteosoma grassii*, Labbé.

fact, worth millions to the human race, how exactly malaria is acquired—a fact which neither Manson nor I had dreamed of before. I telegraphed the discovery to Manson in July 1898 ; and he, in his turn, announced it to a large meeting of the British Medical Association in Edinburgh before the end of that month. In August I was again obliged to discontinue my malaria-researches in order to investigate kala-azar in Assam, and I left India altogether in February 1899. But in July and August 1898, before starting my kala-azar work I succeeded in infecting heavily and experimentally a number of sparrows previously found to be free from infection ; and in my letter of the 6th July 1898 (Memoirs, page 297) I was able definitely to announce the facts to Manson and to the world. This concluded the second stage of the enquiry—the general life-history of the malaria-parasites in mosquitoes.

Few people troubled to see my work in Calcutta ; my own Chief did not visit my laboratory, either then or later. I was still thought to be a visionary, in spite of the evidence which I had collected. Yet my discovery was published in no less than five papers ; namely [29] and [30] on the “ pigmented cells,” in the British Medical Journal of the 18th December 1897, and the 26th February 1898 ; [33] my earlier work on the *Plasmodium relictum*, by Manson, in *ibid* of the 18th June 1898 ; [32] my detailed report with nine plates, dated the 21st May 1898, Government Printing, Calcutta ; [34] the address by Manson to the British Medical Association at the end of July, printed in the Journal of Tropical Medicine for August, the Lancet, 20th August 1898, and the B.M.J.,

PLATE III



PATRICK MANSON

[Face p. 16]

24th September 1898, giving the whole general life-history of the parasites including the infection of healthy birds by the bites of mosquitoes. A sixth report [37], giving full details of the last subject, was dated the 11th October 1898, and was issued by Government Printing after I had left Calcutta in August for the kala-azar work. Numerous copies of [32] were issued to Manson and other distinguished workers on malaria at the end of June 1898; and no one could honestly pretend that my Indian discovery had not been sufficiently made known; but for months scarcely any notice was taken of it, except a scurrilous attack upon me by an Indian Surgeon. I was treated as if I had injured humanity, not conferred a benefit on it; but this was nothing to the abuse which I was about to receive from all sides.

I was able to dissect a few mosquitoes, soon after I had commenced the arduous kala-azar work, but was always dogged by bad luck, and the insects examined by me were always negative, though some of the species are now known to be positive (p. 43). It was found later that one of these species, called *Anopheles rossii* by Giles after me, does not carry malaria at all, at least in Calcutta; and Dr. C. W. Daniels (who had been sent by the Royal Society to Calcutta to help me in December 1898) and I failed entirely to infect them.

Plasmodium falciparum and *P. relictum* were, I understand, the first unicellular parasites which had ever been found to undergo a second stage of development in a second host, as many higher parasites were already known to do. This discovery not only showed

us how what is perhaps the most important of diseases is carried, but also opened a new chapter in parasitology, and established my priority for all similar life-histories, although the facts have not even yet been perhaps fully grasped during the thirty years which have since elapsed. The discovery was perhaps as really important for mankind as the discovery of America.

We left Calcutta on leave to England on the 22nd February 1899, without thanks or notice.

My Indian studies on malaria have been much misrepresented during the last thirty years in the interests of various persons, by people who seem to think that a hypothesis is of equal value to a proof, and by those who do not trouble to examine original papers. I hope that anyone who questions statements in this book will try to verify them, or to disprove them, by personal reference to the original works mentioned in the References.

E. Hartman has recently stated ("Archiv f. Protistenkunde, Bd. 60, 1927) that the name *relicta* is a *nomina nuda* and he seems to attach new names to a parasite which, judging by his coloured drawings, is nothing but the same organism. The parasite with which I was working in Calcutta was very accurately described by Labbé as *Proteosoma grassii* (Archives de zoologie, 1894). The nomenclature of these organisms is very confused.

CHAPTER IV

THE THIRD STAGE, ITALY, 1898

THE object of the third stage of the enquiry was evidently to extend my results fully to cover all the three species of the human *Plasmodia*. For this purpose it was necessary to find the species of mosquito capable of carrying each different species of *Plasmodium* in each malarious country. And there was then no *a priori* reason for supposing that different countries would possess carriers of the same species, or indeed of the same genus (see p. 43). The proper method to follow was the one which I had adopted in the case of *P. relictum* (*Proteosoma*) (page 15) on the 10th March 1898, and indeed all through my investigations, namely, to feed different kinds of mosquitoes on infected subjects and to observe which kind of mosquito became infected by its feed. The first stage of my work had already partially inculcated "dappled-winged" mosquitoes for *P. falciparum* in Secunderabad. The same procedure exactly was now to be followed as before, regarding the human parasites, except that men were to be substituted for birds for feeding the mosquitoes. Only leisure and sufficient opportunity were required—unfortunately I possessed neither after August 1898. Owing to the

zoological affinity of the human and the avian parasites, the mosquito-stages of these organisms were almost sure to be the same or very similar—they have proved to be exactly the same, except for small differences, and to be found in the same parts of mosquitoes by the same kind of dissection, described by me in May 1898 [32].

The task had of course to be undertaken by many observers in all malarious localities, as different species might quite possibly be carriers in different places, and it is perhaps not even yet quite complete. It was first commenced for Italy, after the interruption of my work in August 1898, by B. Bastianelli, A. Bignami, (two Roman physicians) and G. B. Grassi (Professor of Zoology at Rome) in 1898.

I advise anyone who wishes to study the writings of these gentlemen to be very cautious before he accepts all the historical details alleged by them. It has been known for a long time (see for instance Laveran [10*a* and *b*], the letter of Geh. Prof. Dr. R. Koch to me of 10th Feb. 1901, and my Memoirs, pp. 122, 392–399, and 408), that certain gentlemen are not too scrupulous in trying to obtain priority by other means than by anteriority. Such claims should always be compared with the writings of others, if the reader really desires to learn the facts.

The first paper of 1898, which I discuss, was one by G. B. Grassi [35*a* and *b*]. He commences by referring to old work by himself and S. Calandruccio which showed (?) that malaria is *not* carried by certain mosquitoes, but now evidently suggests that it had proved just the opposite, and then mentions the work by

Bignami criticized by me on page 7 ; but does not allude to my refutation of that same work ([25] and page 7). He next mentions the work of Smith and Kilborne [12], and also the discovery that "one of the so-called malaria parasites in birds has for its intermediate host a mosquito." This second passage is found only in [35*a*] and can but refer to my researches. He refers again to my work at the bottom of his first page, as found in both versions of this paper [35*a* and *b*]. In neither case is Manson or myself alluded to by name and no references to our writings are given in order to enable Italian readers to study the facts for themselves—a serious and improper omission.

In both versions, on his first page, he informs his readers that he recommenced his previous researches on the subject since the 15th July onwards. Strangely enough that was a date when I was nearly finishing my malaria-researches in Calcutta by infecting healthy birds by mosquitoes ; and was only a fortnight before Manson announced the whole discovery before the British Medical Association [34] as Grassi himself observes later. Not only, however, does Grassi omit now to mention our names, and also to cite our previous publications (page 16), but he also neglects to observe that we had been at work on the subject for three years already—probably none of his hearers or readers had ever heard of either of us or of our writings. Evidently it was much more important to know that Grassi himself had now determined to take up the subject and to enlighten the world.

When we continue to read this double paper in
S.M.

order to find what he had really achieved with such unexampled originality, we learn only that he was attempting to detect the malaria-bearing species of mosquitoes by comparing the frequency of the different species in malarious and non-malarious areas in Italy respectively by "random-sampling"—a method which no sound scientific worker would have employed. I, myself, had commenced something similar round Ootacamund early during 1897, but soon gave up the attempt for the following reasons. Everyone knows that the number of mosquitoes of any one species anywhere is apt to vary greatly from time to time. There is no *a priori* reason for supposing that a malaria-bearing species need be more numerous in a malarious locality than the non-malaria-bearing species. Evidently the numbers of the latter do not affect the question at all, one way or another. How therefore did Grassi propose to distinguish them from the guilty species simply by estimating the comparative frequency of both? Still further, how could he even estimate the frequency of such a variable quantity merely by visiting any locality on one or two occasions? A residence of months, or even years, would be required by anyone, or better by a party of scientists, who seriously attempt to find the frequency of any one species in any country; and so far as I can ascertain, the whole of this brilliant investigation could have occupied Grassi alone only a few weeks, because we find that both versions of his paper were sent in before the end of September. Evidently a period of only $2\frac{1}{2}$ months from the 15th July at most sufficed such a great mind to clear up the whole of this subject which

we had been labouring at since 13th May 1895. The mere proposal was quackery.

It is astonishing how many brilliant scientists, or at least compilers of text-books, have been deceived by this interesting exploit of their worthy colleague, G. B. Grassi. What exactly did Grassi discover on this occasion? It is somewhat difficult for his enthusiastic admirers to reply, simply because his conclusion given in version [35a] is not to be found in the other version [35b]. Although the Policlinico version [35a] is said to have appeared first (1st October), Grassi stated later that it was really the second version of the paper with some additions which he had found it necessary to make between the despatch of the first and second versions (on the same day!). The first version (Policlinico) is dated at the end "Roma, 29 Settembre 1898"; while the second version (Lincei) is not dated at all, but is said to be in the heading "pervenute all' Accademia prima del 2 ottobre 1898," and is called a "nota preliminare." We find, however, that Grassi in his next article, another nota preliminare, read before the same distinguished academy on the 6th November 1898, states on page 236 that his former article [35b] was communicated to the Academy on the 29th September also, precisely the same date as that given to the first article in the Policlinico version. He must therefore have attached to the Policlinico version *the same date* as that on which he submitted the Lincei version. Grassi himself has always declared, as we said, that the Policlinico version was the corrected later version, but there are those who think from the evidence that both versions must

have been submitted on the same date and probably constituted what sporting men call "hedging," so that if one version proved wrong, the other version might possibly prove right. He could scarcely have found new evidence, requiring a second version, within 24 hours.

The Lincei version leaves it open to the reader to conjecture the exact species of mosquito which Grassi then thought was the guilty species. But his own words on p. 476 of the Policlinico version are "in conclusione, io sono d'avviso che il *Culex penicillaris* e l'*Anopheles claviger* o per lo meno (or at any rate) il *Culex penicillaris* fors'anche (and perhaps) il *Culex malariae*, nella malaria si comportino come la zecca nella febre del Texas" (behave like the tick in Texas fever). The Lincei version [35*b*] does not contain this conclusion.

Another interesting fact is that the date 29 September must have been about the very time that Grassi received or saw several publications from Manson and myself on our work, as admitted by himself in his next paper [36]. See next page.

We must conclude that even Grassi himself was not at that date so very confident regarding the scientific value of his brilliant researches of July to September, 1898. What exactly were the results of these researches? Out of 3 species of mosquitoes which he then concluded in the Policlinico version were carriers of malaria, 2 species were subsequently proved innocent, and out of 7 species which he then concluded were innocent, 2 species proved to be guilty—surely a very ambiguous result for his method which he main-

tained afterwards had really solved the problem of human malaria "indipendentemente da Ross."

Whether the two versions actually appeared first on the dates printed on the original issues (of which I possess copies) I have no means of saying; but whatever the actual dates of issue may have been, it is most extraordinary that the R. Accademia dei Lincei (which corresponds in Italy to our own Royal Society in England) should have allowed one of its own "rendiconti" to have been published with important alterations but with the same title almost simultaneously in a journal, and without any notification of the facts.

That the world might make no possible mistake regarding Professor Grassi's discoveries, he read a second paper on the subject a few weeks later [36]. In this he repeated the conclusions of his Policlinico version [35a], but placed his positive mosquitoes in a different order so that *A. claviger* now comes first (in [35a] it held a second and even doubtful position) while *C. penicillaris* and *C. malariae* (a mosquito like *C. vexans*, to which he himself had formerly given the name *malariae* when he was evidently less sure of the subject even than he was now) came second and third [36, p. 236].

But much more important than such obvious guesswork was the fact that at the beginning of the same paper [36, p. 235] he actually acknowledges having received or seen "alla fine di settembre" (1) Manson's address at Edinburgh [34c]; (2) a "Memoria del Ross," which could only have been my Report [32] containing full details of my technique and results

of my *Proteosoma* work up to May 1898—though, probably with an eye to his future pretences, he takes care not to name it properly ;* and (3) some specimens of “ Grey mosquitoes ” sent by me through Manson, with which were, if I remember aright, some Calcutta *Anopheles*, though he does not mention the latter. He also acknowledges some work of R. Koch. This paper gives a complete refutation of the statements frequently made by those who write on the subject without studying original papers, that Grassi’s work (?) was done without any knowledge of mine.

It was just at this juncture that the late Dr. T. Edmonston Charles, formerly an officer in my Service, commenced to write a series of letters to me in Calcutta from Rome. The first of the series is dated November 4th 1898, and the letters describe fairly closely the progress which the Roman savants were making at that period. With the consent of Dr. Charles I published the whole series later [61] ; and the first five of them will be found copied *in extenso* in my Memoirs, pages 336 to 348 [101]. Near the end of his first letter (Memoirs, page 339) he wrote : “ It has been a cause of surprise to me how very closely they [the Roman savants] have followed all that you have done, and how fluently they talk regarding details of your work.” In his letter of 25th November 1898, Charles describes how he had visited Professor Grassi’s laboratory and shown him one of my specimens. In his letter to me of 8th November 1898 (Memoirs, page 336) Manson had informed me that he had sent Bignami and Charles one of my specimens each.

* It is properly acknowledged by Bignami in [38].

Most probably therefore Grassi had seen my specimen of "pigmented cells," sent by Manson to Bignami long before the date of Dr. Charles's visit to Grassi. However that may have been, Charles now states that on the occasion of that visit Grassi had "before him the British Medical Journal with your paper of the 18th December 1897 [29], and seemed perfectly satisfied that your description of the mosquito referred to the *Anopheles claviger*."

Professor Grassi was, I understand, neither a mathematician nor a medical man, or else he would have understood the futility of these two papers of his [35, 36]. Probably most of his more learned hearers on these occasions merely thought that he was attempting only a "first approximation" to the truth, and did not anticipate that he would ever found a claim to the whole discovery on such feeble speculations, as he subsequently did. Some of his admirers, however, seem to think that he did really deserve credit because he turned out to be right as regards one (*A. claviger*) of his three positive guesses. It is, however, one thing to make guesses but quite another thing to obtain proof. *A. claviger* was definitely incriminated later by my method, not by his, which, as I have shown, was incapable of giving any definite proof at all.

But there is an alternative possibility which must be held in remembrance. When Grassi read his two papers [35, 36] my article [29] had been published already for nine months, and quite possibly Grassi or his colleagues had recognized the probable genus of my "dappled-winged mosquito" from my own description of the spots on its wings and its eggs. We

can scarcely expect these ingenuous but ingenious gentlemen to confess having done so ; but in their first conjoint paper [40] they actually said (see page 33) that they thought my "dappled-winged mosquito" was the *same species* as their *A. claviger*. Certainly Grassi said later that he did not know what the eggs of *Anopheles* were like ; but in saying so he only exposed an ignorance of mosquitoes which I had long suspected, for he only had to dissect almost the first *Anopheles* which he could catch in order to identify my description. If they did not recognize my mosquito by its wings and eggs, we may well ask how they appeared so certain of its species in their later publication [40].

As my first paper [29] on the pigmented cells has been constantly misrepresented by various writers, it may be useful to quote the following sentences from it. The article began :

"For the last two years I have been endeavouring to cultivate the parasite of malaria in the mosquito. The method adopted has been to feed mosquitoes, bred in bottles from the larva, on patients having crescents in their blood and then to examine their tissues for parasites, similar to the haemamoeba in man. The study is a difficult one, as there is no *a priori* indication of what the derived parasite will be like precisely, nor in what particular insect the experiment will be successful, while the investigation requires a thorough knowledge of the anatomy of the mosquito. Hitherto the species employed have been mostly brindled and grey varieties of the insect ; but though I have been able to find no fewer than six new parasites of the mosquito, namely . . . I have not yet succeeded in tracing any parasite to the ingestion of

malarial blood, nor in observing special protozoa in the evacuations due to such ingestion. Lately, however, on abandoning the brindled and grey mosquitoes, and commencing similar work on a new, brown species, of which I have as yet obtained a very few individuals, I succeeded in finding in two of them certain remarkable and suspicious cells containing pigment identical in appearance to that of the parasite of malaria. As these cells appear to me to be very worthy of attention, while the peculiar species of mosquito seems most unfortunately to be so rare in this place, that it may be a long time before I can procure any more for further study, I think it would be advisable to place on record a brief description both of the cells and of the mosquitoes.

“ The latter are a large brown species, biting well in the daytime and incidentally found to be capable of harbouring the *Filaria sanguinis hominis*. The back of the thorax and abdomen is a light fawn colour, the lower surface of the same and the terminal segment of the body a dark chocolate brown. The wings are light brown to white and have 4 dark spots on the anterior nervure. The haustellum and tarsi are brindled dark and light brown. The eggs—at least when not fully developed—are shaped curiously like ancient boats with raised stern and prow, and have lines radiating from the concave border like banks of oars—so far as I have seen, a unique shape for mosquito's eggs. This species appears to belong to a family distinct from the ordinary brindled and grey insects ; but there is an allied species here, only more slender, whiter and much less voracious. My observations on the characteristics of these mosquitoes were not very careful, as when I first obtained them, I did not anticipate any difficulty in procuring more.”

Probably the “ large brown species ” was *A. stephensi* and the “ smaller allied species ” was *A. culicifacies*, Giles.

“Thinking, however, that I may have overlooked these delicate cells in former dissections, I have again examined a large number of brindled and grey mosquitoes fed on malarial blood. Their stomachs certainly contained no such cells.”

The rest of the paper describes “the pigmented cells” more exactly, gives drawings of the cells made by Manson, and quotes remarks on the matter by Smyth, Manson, Bland-Sutton, and Thin.

I regret that I could not at the time give a proper entomological description of these two species of mosquitoes because I could not then obtain any information whatever on the entomology of Indian mosquitoes, as I have said (see also my *Memoirs*, pp. 264, 265).

Whether Grassi really believed that his method used during the summer of 1898 was of any value at all, or whether he merely pretended to have found some new method in order to establish a claim for himself in scientific history, I cannot say; but I suspect that the former supposition was correct; and certainly he has successfully misled a number of writers on the subject.

When exactly he and his colleagues first recognized my “dappled-winged mosquito” to be an *Anopheles*, I do not know; but they certainly did so before their paper [40] and may have done so months previously. Indeed it is possible that he always included *A. claviger* among his three positive guesses simply for this reason. These gentlemen appeared to study the *British Medical Journal* fairly closely, as proved by Bignami’s paper discussed on pages 6 and below, and Grassi might have seen and studied my papers [29, 30] even

before he set out on his original and independent investigation on the 15th July 1898.

Another point ought to be noted. In the passage quoted from [35a] on page 24, Grassi expressly states that his three positive guesses "behave like the tick in Texas fever." None of them do so; and Grassi was still under the influence of Bignami's mistaken analogy of 1896 (page 6).

With admirable perseverance but on the strength of the same hypothesis, the latter had long been trying to infect men experimentally by mosquitoes collected from marshes, since 1894, but without success. On the 3rd November 1898, however (the day before Charles's first letter to me), Bignami's third experimental subject, Sola, showed *Plasmodia* in his blood (as Charles stated). I confess that this result of Bignami's astonished me considerably at first; but when I came to examine his detailed paper [38], I found the explanation, that, though he does not confess it as frankly as might have been wished, and though his paper was very long, very involved, and full of hypothetical considerations, he had made a complete change in his procedure since the 26th September ([38b], p. 1542). That was just about the date when the later papers of Manson and myself [32, 34] were received or seen by Grassi "alla fine di settembre" (page 25). Bignami, who mentions both these papers explicitly and honestly in his footnotes, had been working with *larvae* brought from malarious marshes and hatched into adults in his laboratory for biting his subjects (in accordance with his and Grassi's old mistaken hypothesis that mosquitoes "nella

malaria si comportino come la zecca nella la febre del Texas ” (page 31) ; but now in October he began to have his experimental subjects bitten by mosquitoes which were brought directly in the *adult* stage from an intensely malarious locality (Maccarese), where some of them might have been infected by biting infected persons previously. That was quite a different matter, though he does not confess it clearly and candidly. Really, the whole experiment now became simply a repetition of my experiments on the infection of birds by mosquito-bites made in India in the previous July-August—though few of the numerous experts who have written on the subject, especially in England and America, seem to have detected the simple artifice employed in Bignami’s paper. As was to be expected, Sola became ill on the 1st November, and showed *Plasmodia* in his blood on the 3rd November, when quinine was administered. It was not ascertained which of the mosquitoes employed contained “ pigmented cells ” or “ germinal threads ”—presumably because the Roman savants did not then know how to look for them—though they discovered this a few days later quite “ indipendentemente da Ross.” How clever of them ! It took such a slow observer as myself three years to make the same discovery.

There are many passages in this same prolix paper by Bignami, on historical points, with which I profoundly disagree, but I will not take the trouble to expose them in detail. He also mentioned only my work on the malaria of birds and ignored my fundamental paper [29].

Another paper, on the staining of the flagellate

bodies, by Bignami and Bastianelli, appeared in the *Lancet* on the 17th December 1898. It is also unnecessary to analyse this article, though I do not agree with some passages in it.

Bastianelli, Bignami, and Grassi now began to work in collaboration and to employ the proper method which I had been using and had fully described months previously—to feed different kinds of mosquitoes on an infected subject and then to find by dissection which of these mosquitoes contained the “pigmented cells” which I had also been describing for a year. Success was claimed immediately, and they said they found the cells in several *Anopheles claviger* and published a conjoint paper within a few days [40] in typical haste. As I have said, I cannot ascertain exactly when they *first* recognized my “dappled-winged mosquitoes” to be *Anopheles*; but they were now even so simple as to think that my mosquitoes had actually belonged to the *same* species as their *Anopheles claviger* vel *A. maculipennis*.

But even in this first conjoint paper of theirs they maligned my discovery by pretending (contrary to the second printed sentence of my paper, see page 28) that my mosquitoes (bred in bottles from the larva) may have previously bitten infected animals. This was a deliberate *asseveratio falsi*.

They themselves gave no assurance that the same source of error which they falsely imputed to my experiments may not have vitiated their own. They vouchsafed no evidence that their “pigmented cells” had really been derived from the parasites in the subject except that they grandiloquently state that

they judged this to be the case “*con tutta sicurezza*,” for which they really relied on the first and second stages of my work which they simultaneously condemned by a false statement, as being inaccurate, or which they neglected to notice at all. No wonder that Robert Koch subsequently concluded in his letter to me of the 10th February 1901 (my *Memoirs*, page 408) that the whole article was pure invention, while Laveran afterwards described their doings as “*mesquins et malhonnêtes*” (*ibid.*, page 410). This paper in Italian and in a translation by Charles, will be found *in extenso* in my *Memoirs*, pages 348, 349.

In this country it is held to be a point of honour for all scientific workers to acknowledge as fully as possible the previous and the often more difficult efforts of others ; but there was no sign of obedience to any such obligation on the part of these authors. It would be tedious to examine, or even to mention, all the chicanery, both *suppressio veri* and *suggestio falsi*, with which they endeavoured to persuade their readers, few of whom had probably ever seen or heard of my writings or could obtain copies or read them, that their work was original and that mine was erroneous.

Grassi, later, even attempted to obtain a Nobel Prize on the strength of his pretences and did obtain some pecuniary rewards ; but the Nobel Prize was awarded to me instead at the end of 1902, after careful scrutiny of the literature on both sides.

Grassi even had the audacity to pretend later that it was he who proved the innocence of *Culex* and *Aedes* mosquitoes in the winter of 1898-1899, in spite of the fact that I had worked at them for three years

before he even began his "studies" of 1898. Even several years later he remained so ignorant of mosquitoes that he tried to ridicule me for estimating the minimum age of one of them (Memoirs, page 412).

The laborious first and second stages of my work were mostly ignored by them from the first, and they pretended to have found the "pigmented cells" "indipendentemente da Ross" within a few weeks by what must have been a kind of miracle. Of course really it was my previous work which showed them what exactly the cells were like to look at and where exactly they were to be found. Fortunately, as I have shown, there is complete evidence, even from their own writings, that they were fully acquainted with my writings and those of Manson long before they obtained or alleged any genuine successes of their own—which we can only infer that they did obtain from the fact that their later results were proved to be correct by the more reliable labours of others. All their work of 1898 is vitiated by the reader's suspicion that they were desperately anxious to find something, no matter what, independently of me, to enable them to lodge a claim to the discovery, no matter how, which would appear valid to the ignorant.

But in my opinion, these efforts were probably due to Grassi and not to his colleagues, who I have endeavoured to persuade myself were comparatively innocent. No wonder that Robert Koch wrote to me in the letter already quoted (10th Feb. 1901) (my Memoirs, p. 409) "obwohl ich Grassi für einen Schuft und einen Rauber auf wissenschaftlichen Gebiete halte."

I agree fully with Koch : he, like Laveran, Manson, and Lord Lister knew the facts.

It is curious how few writers have understood either the magnitude of the original problem or the steps by which it was really solved. Though they generally give alleged bibliographies these often omit necessary entries or do not adequately date all of them. Thus the reader often has no means of judging exactly when a given observation was first reported ; and the histories of entirely different subjects are often so badly arranged that the reader cannot ascertain from a given book which observation came first in point of date. It is evident that many writers possess no adequate experience of a subject, while others are easily gulled by the most preposterous assertions.

One often wonders how much longer honest research will continue to be made when it is so often falsified by ignorant, careless, or interested historians. As I have already pointed out many times, the life-histories of the human *Plasmodia* in mosquitoes are almost exactly the same as that of *Proteosoma*, though Grassi and his friends ignore, or try to obscure, this fact.*

From that year (1898) Grassi constantly pursued me with the rancour of a tyrant towards his victim, or of the wolf towards the lamb. His book of 1900 [57] is a masterpiece of misrepresentation containing 31 pages of spurious abuse of R. Koch and myself, and a defective bibliography, and easily gulls unwary British and American readers and many others. It has forgotten all about the *Culex penicilaris* and *Culex malariae*. It gives small credit even to his own colleagues. He

* I give these small differences in [52].

himself is the Achilles of his own Iliad and I am dragged, a corpse, round the walls of Troy. A few months after that book appeared I tried to defend myself humbly in the pages of the Italian Policlinico, but was easily routed by Grassi's replies. It seems that I did not possess enough intelligence to make so great a discovery, which had been reserved for him. The book was soon translated into German, and is much admired by those who know little of the subject.

It was this book which contained, on page 31, the inexcusable falsehood that he had discovered the "*Anopheles malariferi*" independently of me, whose work on the malaria of birds was published almost contemporaneously with his preliminary note (sic). He even repeated similar mendacity in a lecture which he delivered before the Queen of Italy on the 25th March 1900 (see the tract *Ancora le scoperte del Prof. G. B. Grassi sulla malaria* by Prof. S. Calandruccio, published by H. Tip. Barbagallo e Scudari, Catania, 1901).*

Two years later, when he found that a Nobel Prize was not coming to him after all his meritorious efforts, Grassi published another (little) book (Rancati, Milan, 1903), said to contain Italian translations of documents concerning the discovery of malaria and mosquitoes. It was widely distributed throughout the world. Will it be believed that vital passages are entirely omitted without explanation from the Italian translations of

* Readers of Grassi's *Studi* have told me that they accepted its statements because it is dedicated to Manson ; but the latter explained later that he had accepted the dedication before he had seen the work. See also the next paragraph.

my fundamental paper [29], and that Grassi's book is dedicated, *without permission*, to Manson, evidently to get Italian readers to accept its statements about me on the strength of that dedication? But Manson proclaimed this fact publicly and repudiated the dedication in a letter to the *Lancet* and the *British Medical Journal* of the 28th March 1903. In short, this book of Grassi's, like much of his work, was a deliberate fraud on Italian readers. It even attacked Lord Lister, who had studied the subject carefully in order to report as President on my work to the Royal Society in 1902. Error repeated after correction becomes fraud.

Shortly before his death in 1925, Grassi belaboured me again in the pages of *Nature* of March 1924, and, I believe, elsewhere with the help of English friends. It was obviously impossible to deal with such a disputant, and I refused to reply, and later he threatened to sue me and Sir Arthur Shipley who had attempted to defend me. It is really shameful that men, who, after all, have only attempted to benefit humanity without hope of reward or even of remuneration, should be subjected to treatment which in matters of "real property" would probably lead the offenders to prison.

I regret being obliged to infringe the good old "De mortuis nil nisi bonum"; but the integrity of history must be maintained in spite of it.

It is unnecessary to take the trouble of analysing other papers by these prolific writers—though I frequently disagree with their statements and do not admit their claims. More details about Grassi's

methods will be found in the article by his whilom colleague, S. Calandruccio, in the *Journal of Tropical Medicine*, 1st July 1901, and by me in *Science Progress*, October 1925, page 311.

I conclude therefore as follows :

1. That these observers had detected the scientific name of the genus (*Anopheles*), if not the species of my "dappled-winged mosquitoes" from my description published nearly a year previously [29].
2. That they subsequently succeeded in growing the "pigmented cells," also described by me nearly a year previously in the same paper, in a species closely allied to mine.
3. That still later, by lucky experiments, they succeeded in growing also the quartan and tertian parasites in the same and in other species of "dappled-winged mosquitoes."

They thus accomplished what I have called the third stage of the investigation for Italy—but only for Italy. On the other hand, their credit for this work was much diminished by the facts :

1. That the method of random-sampling first used by Grassi in his alleged endeavour to distinguish the malaria-bearing species of mosquitoes in Italy was (a) scientifically incapable of yielding any certain results ; and (b) led only to his suspecting three different species of mosquito, two of which species he himself subsequently admitted to be innocent.
2. That they pretended their work was original (a) by publishing many false statements regarding my previous work ; (b) by omitting reference to fundamental details of it ; (c) by pretending that the development of the human parasites in mosquitoes

was quite different from that of *P. relictum* (*Proteosoma*) of birds.

3. The pretence of Grassi to prove that he arrived at the "*Anopheles malariferi* indipendentementa da Ross" has probably been the most impudent fraud ever attempted against the scientific community ; and the success with which so many of that community have been gulled by him shows with what little care they examine original papers and work. My experiences at the hands of these gentlemen have not added to my respect for them ; and I have often regretted ever having touched such a subject as medical research.

I doubt whether Grassi ever made any real discovery in this subject ; but I do not bring any charge against either Dr. Bignami or Dr. Bastianelli.

See also notes by me in the Journ. Trop. Med., 1st September 1926 and 1st October 1926 (2 articles), and 1st April 1927.

CHAPTER V

THE THIRD STAGE, FREETOWN, 1899

WE arrived in England in March 1899. In the previous year Manson (now Medical Adviser to the Colonial Office) had informed me by letter that Rubert Boyce, Professor of Pathology in University College, Liverpool, was trying to follow his (Manson's) advice and to establish a School of Tropical Medicine in that city and college, and Manson advised me to apply for the post of Lecturer on Tropical Medicine in the proposed school. I did so, went to Liverpool to see Boyce and Alfred Jones (a Liverpool ship-owner who was financing Boyce's venture), and was duly appointed to the post shortly afterwards at the salary of £250 a year in addition to my pension of £292 from the Indian Medical Service, in which Service, of course, I was obliged to resign my Commission at the same time.

Dr. Laveran had long asked me for an account of my work, for the Académie de Médecine of Paris. I finished my article in Calcutta on the last day of 1898. It was presented to the Académie in French on the 24th January 1899; and was "reported" upon by Laveran himself on the 31st January. His "report," which reached me later in England, did me full justice;

and in my paper I repeated the acknowledgements which I had made to Laveran and Manson for their help, in my *Proteosoma* Report. I also praised Grassi's work ; but that was before I had had time to study it as carefully as I subsequently found was necessary.

The new Liverpool School of Tropical Medicine was formally opened on the 22nd April 1899 by Lord Lister, the inventor of aseptic surgery, then President of the Royal Society. Manson had kept him in touch with our work ; and he now began to study it thoroughly. Meantime I had long been maturing my intention to continue studying the third stage of my investigations somewhere in West Africa during the University vacation. I particularly wished to apply my methods for determining the carrying species of mosquitoes in some tropical colony, and finally fixed upon Freetown, Sierra Leone—called “ the white man's grave ”—for this purpose. Dr. H. E. Annett, who assisted me as Demonstrator of Tropical Pathology at Liverpool, and Mr. E. E. Austen, then Dipterologist at the British Museum of Natural History, Cromwell Road, London, volunteered to accompany me. We received the consent of the School Committee on the 14th June ; Messrs. Elder Dempster (Mr. Alfred Jones's firm of ship-owners) kindly promised us all free passages to and from Freetown ; and we left Liverpool on the 29th July 1899 in the *S.S. Fantee*.

We arrived at Freetown on the 4th August, and one of the first things we saw there in a shop was a “ dappled-winged mosquito ” very like, in wing-markings, body-shape, and attitude when seated, to the large “ dappled-winged mosquitoes ” in which I first found

the "pigmented cells" in August 1897; next day one of the station doctors sent us some more of these mosquitoes of two varieties, large and small. Austen quickly recognized the former as *Anopheles costalis* and sent the small variety home, where it was described and named *A. funestus*, as a new species by Colonel Giles, I.M.S., then working at the British Museum and later the author of "Gnats or Mosquitoes," which, I understand, was the first textbook on the mosquitoes of the world then recognized.

Subsequently, both species have been found throughout Africa as far east as Mauritius, but *A. funestus* now appears to be the same as *A. listoni*, in a few individuals of which I had failed (by bad luck) to find "pigmented cells" in the Indian terai when I was on the way to Assam in August 1898 (p. 17). Now, in Freetown, a year later, I found the cells at once in one out of five *A. funestus* examined on the 13th August 1899. From the colour and character of the pigment in the "pigmented cells" in this insect, I judged that it had been derived from a case of human mild tertian fever.

At that time, there was no evidence at all for asserting that the human parasites could develop only in mosquitoes of the genus *Anopheles*, and less for asserting that all *Anopheles* could carry human malaria. In fact, Daniels and I had failed in infecting a few *A. rossii* in Calcutta in the winter of 1898-99, just as I had failed for three years with a number of species of *Culex* and *Aedes*. The Italian observers had, I believe, worked only with the three species of *Italian Anopheles*; and it was altogether doubtful whether

their researches on other Italian genera during that winter and the spring following had been at all adequate. But in my voyage from India in February-March 1899, I had thought of a much better reason for incriminating *Anopheles* only than any reason they gave, namely that, unlike *Culex* and *Aedes*, the *Anopheles* in India at least seemed always to breed in terrestrial waters and not in tubs and pots, while malaria had been connected for centuries with terrestrial waters also. I determined therefore to begin with studying only *Anopheles* in Freetown—where we could spend only a few weeks before returning to England.

Shortly after landing, we heard that there was a bad outbreak of malaria then proceeding at the Wilberforce Barracks, a few miles out of Freetown, among some coloured troops from Barbadoes, who were stationed there. Now Barbadoes was then free from malaria * and the troops were consequently suffering rather heavily, not being immune to it.

We went there on the 17th August and found the regimental hospital full of cases of all three kinds of malaria, while the walls were dotted with numbers of *A. costalis* gorged with blood and evidently feeding on the patients (there were no birds or animals there for them to feed upon). On examination during the next few days, we found all three species of malaria-parasites in the blood of a quarter of the men of the detachment, while a quarter of the *A. costalis* were also infected with the corresponding "pigmented cells."

* Unfortunately malaria has been recently allowed to enter that island.

It was easy to distinguish the species of the "pigmented cells" in the mosquitoes simply by the different appearances of the pigment-clusters in each (p. 36). Out of 109 mosquitoes dissected by us on that occasion, we actually found the "pigmented-cells" in 27. Also, two *A. costalis*, caught by hand-net and kept alive in test-tubes, were fed on cases and subsequently showed "pigmented cells" of the appropriate size. In fact, fate or chance, so often churlish to me in India, now compensated me by giving me an almost perfect object-lesson in Africa.

We telegraphed our success to Mr. Alfred Jones, and I described all our work in Freetown in four anonymous articles (it was "bad form" for doctors' names to appear too much in the press), in the British Medical Journal (9th, 16th, 30th September and 14th October 1899); and after doing much more work in Freetown, to be described in the next chapter, we started again in the same ship on the 17th September for the autumn term. But the authorities in Freetown refused to allow me to feed any more of their valuable mosquitoes on their infected troops: apparently they were taking no precautions at all to prevent their soldiers from becoming infected from their mosquitoes but strongly objected to their poor mosquitoes becoming infected from their soldiers.

Of course my work in Sierra Leone was simply a continuation of my work in India; there was at that time no reason at all for supposing that either Italian or Indian results, as regards the guilty species of mosquitoes, would follow in West Africa. But that did not prevent Grassi from pretending later with

great impudence that all my Sierra Leone results were plagiarized from his Italian findings or inventions. Needless to say, our West African observations would have been exactly the same if Grassi and his friends had never been born.

CHAPTER VI

THE FOURTH STAGE, FREETOWN, 1899

ON the 16th February 1899, just before I left India, I sent to the Director-General a concluding report on "the subject of the practical results as regards the prevention of the disease (malaria) which may be expected to arise from my researches."

This report was published without a date and with a changed title in the Indian Medical Gazette for July 1899 and opened the fourth stage of the investigation, for which indeed I had troubled to make it at all—how best to apply the new knowledge of malaria for the saving of human life and health on a large scale. This stage is still being worked at all over the world and requires much knowledge, both general and local, of mosquitoes and their habits, and of malarial diseases.

While I was searching the men and the mosquitoes of Wilberforce for malaria-parasites, Dr. Annett examined the puddles of Freetown for the larvae of *Anopheles*, made a map of the principal breeding-pools, and wrote several excellent articles on our work (*Lancet*, 1899).

Lord Lister was determined to learn all about my work and wrote me many letters enquiring into every

detail. The following epistle from me to him, written while we were on the return journey from Sierra Leone, will perhaps best explain our work in West Africa on that occasion.

S.S. Fantee,
2nd October 1899.

MY DEAR LORD LISTER,

Very many thanks for your letter of the 5th September which we received before our leaving Sierra Leone. I had intended to give you our further results regarding malaria before but finally determined not to trouble you too much with letters until our work was complete. We are now on our way home again, and I sit down to tell you our final conclusions.

I said we had found human zygotes in two species of *Anopheles*. The fever among the troops at Wilberforce continued to yield naturally the most perfect evidence regarding the diffusion of malaria. We found that about 25% of the men had Haemamoebidae, mostly in small numbers, in their blood, though very many of them frequently took quinine. The only species of mosquito present was the large variety of *Anopheles*, and out of 108 of these which were carefully examined we found the same parasites in their various stages in 26. Unfortunately when we first went to Wilberforce we caught many more of the insects than we could examine, that is, we killed nearly all the *old* ones. Later we found that fresh insects arrived only in small numbers, and of these fresh insects a smaller percentage, as was to be expected, were found infected after feeding. The total gives about 25% of infected insects—quite sufficient to account for all the fever in the Wilberforce barracks. We were very fortunate in obtaining such a remarkable object-lesson, which seemed to have been arranged by Nature herself for our instruction.

As soon as we were satisfied that the local *Anopheles* carry malaria, we threw all our efforts into studying the

habits and distribution of the larvae. Things turned out just as I said they would in my inaugural lecture. We found that *Culex* breeds in pots and tubs of water, *Anopheles* in pools on the ground. Any old vessel, broken bottle, empty gourd, or biscuit tin suffices, when full of rain-water for *Culex* ; but only in a single instance did we find *Anopheles* in such and that was in an old tub full of green water-weed. At first we had considerable difficulty in finding *Anopheles* larvae at all, but we shortly learned how to detect their pools at sight. The results obtained are, I think, most interesting, both theoretically and practically. Only certain kinds of puddles suit the larvae. Puddles which dry up too quickly, which are apt to be scoured out by heavy rain, and which contain small fish, do not suit them. We kept a number of puddles under constant observation in order to study the habits of the insects. It was most interesting to observe that while one puddle would always contain larvae, another one close to the first would never contain them. The reason was that the first would fulfil these conditions and the second would not do so. A large number of pools in Sierra Leone are apt to be scoured out by the torrents of rain which fall there ; another large number dry up within a few hours after rainfall, while a few of the larger pools contain little fish. The upshot is that comparatively few puddles are suitable for the larvae, and these are situated either in the ditches by the side of the *flat sections of road*, or in two localities where small runnels of water ooze from the ground and run over flat areas of rock or soil containing numerous small hollows. In the latter localities the puddles, being fed by the runnels of water, are almost permanent during the rains, and contain masses of algae. Here the larvae exist in great numbers. In fact they eat the algae, as was to be seen at once on a microscopic examination of the contents of their stomachs, as well as by watching the little creatures feeding. We ascertained, both by experiment and by observation,

that complete desiccation of a puddle kills the larvae, though if sufficient moisture remains they are uninjured. As rain falls almost every six hours at Freetown during the wet season, it follows that very small pools can harbour the larvae during this season. On one occasion, however, we had four fine days. Nearly all the puddles dried up. Then came heavy and continuous rain and the puddles filled again ; but at first the freshly filled pools contained no larvae, and it was not until 48 hours that we found larvae again—all being extremely small ones, evidently just hatched from eggs laid by the adult *Anopheles* living in the neighbouring houses.

It appears to us that these laws give an almost complete explanation of many familiar theories regarding malaria—such that it springs from stagnant puddles, that it is connected with rainfall (in Freetown the height of the rains is the most malarious season), and that it disappears in consequence of drainage of the soil. You will remember the theory that malaria can rise from decaying rock. It was most remarkable to see the larvae in small pools in the hollows of certain flat, worn old rocks. Again, you will remember the theory that digging the earth causes malaria. This had actually been noticed at Freetown during the construction of the railway. On examination, we found that the railway embankment had produced *Anopheles* pools here and there along its course! It is easy to see that digging the earth may often produce holes which when filled with rain-water may harbour *Anopheles*. The old theories were then fairly right after all ; but it is not the malaria germ itself which rises from stagnant water, but the *carrier* of the germ. Had *Anopheles* bred in pots of water like *Culex*, the old theories would have remained unsatisfied. On the other hand, the fact that *Anopheles* breeds in pools seems to my mind to reconcile the mosquito theory almost completely with known facts regarding malaria—and does so in a particularly beautiful manner.

Of course these observations almost cut the ground from under the feet of the argument that malarial fever may be produced in other ways than by the bite of the mosquito—they seem to me to give a sufficient explanation of the facts upon which that idea was founded. It is scarcely necessary any longer to imagine that the germ rises from the soil and is inhaled and so on ; while on the other hand such a theory would imply another life-cycle in the *Haemamoebidæ*—whose history is already complicated enough. One might as well argue that tape-worms are acquired in other ways than by eating flesh containing cysticerci.

But two important questions remain. First, can any *Culex* carry human malaria ? This I fear we have made no attempt, on account of limits of time, to answer. The R.A.M.C. threw difficulties in the way of our making mosquitoes bite their patients ; so that we were obliged to abandon experiments in this line. But a systematic study of the question is certainly demanded. We found several species of *Culex* in puddles together with *Anopheles* though the larvae were not nearly so numerous. At any rate I am far from being convinced yet that *Culex* is quite innocent.

Secondly, can malarial fever be acquired in uninhabited places ? You discussed this in your letter of the 31st July and suggested that animals may be intermediary hosts. After all, there is so little evidence to show that malaria can be acquired in uninhabited places. Travellers, for instance, must pass through inhabited villages before reaching deserts and can easily be bitten by infected mosquitoes *en route*. But I quite agree that it may be possible, and also that animals *may* be vicarious intermediary hosts. At Wilberforce there are no animals at all. Here simple communication by the *Anopheles* from man to man suffices to explain all the fever, we think ; and if it suffices there why not elsewhere ? By the by, we found no black spores except one doubtful instance, in all the Wilberforce *Ano-*

pheles. My doubts regarding these bodies have grown graver than before.*

Now for the practical results to be expected as regards the possibility of extirpating *Anopheles*. We all agree that if the local authorities will set themselves heartily to the task, Freetown can be freed of the insects at a trifling cost. We carefully made a map of the whole town, giving all the *Anopheles* puddles. There are only about 100 of these all together, lying mostly in clusters. All could be drained at little cost and most could be swept out with a broom. We tried kerosene oil; it was quite successful. Dr. Ould is now trying tar. In fact we can see no difficulty in destroying the insects wholesale, and if such destruction were kept up for a few months it seems probable to us that the prevalence of the insects in Freetown would be dealt such a severe blow that it would take them a long time to recover their ascendancy. The Governor, Major Nathan, has been so good as to adopt our views and has organized measures against the *Anopheles*—but of course we cannot answer for the carrying out of these measures.

Dr. Strachan, Chief Medical Officer of Lagos, who travelled out with us, has found hosts of *Anopheles* there. He has also discovered the larvae in roadside puddles and is destroying them with oil. From specimens which he has sent us, they are the same as the Sierra Leone *Anopheles*. We have also obtained a similar insect from Opobo. Hence it appears probable that this insect is the principal cause of the West African Fever.

Dr. Ould, who arrived at Sierra Leone a fortnight ago, is now going down the coast *to raise the alarm* against *Anopheles* and will then work at Lagos with Dr. Strachan. I firmly believe that a general massacre of *Anopheles* in the principal coast towns would be quite practicable and would result in at least a considerable reduction of the fever there.

* See p. 55.

Austen was attacked with the aestivo-autumnal parasite but has recovered speedily. He slept without mosquito-nets about a fortnight previously (for one night) but does not remember having been bitten. Of course one may be often bitten without knowing it.

In your letter of 31st July (which I have not yet fully answered) you refer to the gametocytes of *H. Danilewski* of pigeons, jays, etc., being crescentic, while I place this parasite in the genus *Haemamoeba*. I characterize this germ by the gametocytes being of the same shape as the sporocytes (whatever that shape may be); while in *Haemomenas* the gametocytes have a special (and, as it happens, crescentic) shape. In *Haemamoeba danilewskii* of jays, etc., both sporocytes and gametocytes are crescentic certainly; but they are both alike and their shape is evidently due to exigencies of space. In *Haemomenas praecox*, the sporocytes are spherical, while the gametocytes have quite a different shape—truly crescentic. It was difficult to retain the word *Laverania*.

I am sorry for the slip about the date of MacCallum's discovery. The hint about cultivating the black spores is very good. I will write to Daniels about it.

You will understand that there will be some difficulty in moving the inertia of the medical profession in these parts in the direction of destroying *Anopheles* larvae. We have thought it best to advertise the discovery of the "malarial mosquito" somewhat freely on this account. I hold that the subject is one of those with regard to which the public should be taken into confidence. Unless this is done progress, I am convinced, will be very slow. For example, the microscope is not even yet habitually employed in the diagnosis of malaria fever in our tropical colonies, although microscopic diagnosis is easy and in many cases indispensable, and Laveran's discovery has been made for twenty years.

I hope Dr. Strachan at Lagos will take energetic measures

S.M.

E

against *Anopheles* there. He seems interested in the subject and will have Dr. Ould to help him.

I fear I have written you an inordinately long letter, but think that you may like to have pretty full details. Our report will not be ready for some time.

Want of time has prevented us from doing any good cytological work. The three human parasites are distinguishable by their pigment in the zygote stage—that of tertian being light brown, of quartan, dark brown, of aestivo-autumnal, black. The capsules of all seem to be a little thinner than the capsule of the zygotes of *H. relictæ* (Proteosoma). Otherwise there seems to be no difference at all. The blasts [germinal threads] seem shorter and thicker and can often be seen within the lumen of the duct of the salivary gland.

I have reported our results also to Professor Ray Lankester, Dr. Manson and Dr. Laveran.

We are trying to bring some *Anopheles* home with us alive, but I fear they will die before our arrival.

Believe me,

Yours sincerely,

RONALD ROSS.

On the 22nd August Dr. Van Neck, a Belgian student at the Liverpool School, attached himself unofficially to our party in Freetown; but there was no room for him in the barracks on Tower Hill, where we were allowed to live by the courtesy of the General, and Dr. Van Neck was consequently obliged to find quarters in one of the Freetown hotels, where he was shortly taken very ill and became useless for more work at that time. About the same time Dr. Fielding-Ould, another student of the School, was sent out by the Committee of the School to help us; and, when we were obliged to return home, he proceeded down

the Coast in order to preach the new mosquito-malaria gospel. We ourselves had originally hoped to do this, but our success at Freetown had kept us busy there. Before we left I presented a report to the Governor, Major (now Sir) Matthew Nathan, R.E., on our work, for the future use of the Colony. The third paragraph of the above letter to Lord Lister gives briefly the points of fundamental importance concerned with the habits of *Anopheles* larvae in relation to malaria up to then observed. The names used in the letter will be explained in the next chapter.

The "black spores" which I thought at first might be stages in the life-history of the parasites in mosquitoes had been found by me in some uninfected mosquitoes before I left India.

CHAPTER VII

WAITING, 1899

WE were home again early in October 1899. Some students were attending ; and I had many duties to continue or to commence. Among the former was the task of taking microphotographs of my Indian and African specimens of malaria-parasites in mosquitoes, many of which were fast perishing. Some of these microphotographs I made into lantern-slides for lectures, and have used them frequently during the last thirty years—they are still in my possession.

More important was the necessity of consulting experts on the zoological bearings of my observations—a duty which I had attacked directly I went to Liverpool, because, as I have said (pp. 1, 14, 47) my work had been done for the sake of the Public Health only, and I was very conscious of the defects, or rather wants, of my zoological knowledge, both general and special. Fortunately the Professor of Zoology at University College, Liverpool, the late Sir William Herdman, F.R.S., proved himself very kind and willing to help ; and with the additional assistance of several books, I wrote a short paper which appeared while we were in Freetown [47]. In this I adopted the name of *Haemamoebidae* (Wasielewski) for the

whole family of these blood parasites in men and animals, which I divided into two genera, *Haemamoeba*, Grassi and Feletti, and *Haemomenas* mihi. Latterly, however, I have adopted the names which are more generally used and which I believe hold priority, namely Plasmodiidae for the whole group, containing a single genus, *Plasmodium*. Grassi and Feletti had produced considerable disorder in the arrangement of these parasites by confusing the crescentic shape of the sexual forms of *Plasmodium falciparum* with the somewhat similar shapes of *Plasmodium danielwskii* of crows and pigeons, really due to the fact that the nucleus of birds' blood-corpuscles compels parasites growing in these corpuscles to take a dumb-bell figure somewhat like that of the "crescents." (See p. 53.)

Later on I laboured also at the physiological nomenclature connected with these unicellular creatures; but my suggestions were speedily swamped by those of Schaudinn which now hold the field of popular approval, but which I do not admire. The nomenclature employed in my Indian writings had been purely provisional because I could not then obtain any information or advice on the subject. I now use the following terms:

Microgametocyte = the male sexual parasite giving origin to the microgametes = sperms.

Macrogametocyte = the female sexual parasite giving origin to a single macrogamete = an ovum.

Zygote = a fertilized ovum = "pigmented cell" when fixed in the stomach tissue of a mosquito.

Protospores = the first progeny of a zygote = germinal threads, germinal rods, sporozoids, sporozoites, etc., found in the salivary glands of mosquitoes.

Deuterospores, tritospores, etc. = second and third generations of spores in vertebrate blood.

Heterospores = a later sexual progeny of the parasites in vertebrate blood.

Sporocyte = a cell full of spores.

I was again much disappointed that while we were in Sierra Leone we had no time to examine the cytology of the mosquito-cycle of the Plasmodiidae, although we took the proper staining materials with us and obtained many fresh dissections of infected mosquitoes there. We could not do the work after our return to Liverpool because fresh specimens were generally required for this purpose. More exact studies of the cytology of the parasites in mosquitoes would have formed a useful addition to my observations; but I was again obliged to leave the subject to the Roman savants who always had plenty of infected mosquitoes near at hand and had already done the work.

On the 25th November 1899 I lectured to the Liverpool Chamber of Commerce on the subject of our recent visit to Sierra Leone. After quoting Martin Tupper's line about Columbus, that he "gave to man the godlike gift of half a world," I added, "similarly it may even happen that such a wild idea as killing mosquito-grubs to prevent malaria may assist in giving to civilization the gift of another half a world—the tropics. We never know, when we plant one of the seeds of science, into how great a tree it may grow some day."

About that time my friend Dr. G. H. F. Nuttall wrote a book and a series of papers on the malaria-mosquito discovery [51] and pointed out that many

of my observations on mosquitoes had been made previously. That of course always happens with each new discovery, even with the original discoveries by my Roman friends. Not being a trained entomologist, I had never heard of these anticipations.

Before the end of 1899 I contributed an article on Malarial Fever to the well-known Medical Annual of 1900. Next March, Manson wrote to me: "I am very grateful to you for the full justice and more than justice you have done me as regards my very small share in the mosquito-malaria theory." But his friends (including myself) will scarcely allow the memory of him to escape so lightly from the honour in which the world holds it, not only for his great induction but for the constant help that he gave to my work during a very trying time.

Directly I returned from Sierra Leone I wrote a little book for the purpose of helping residents in malarious countries to escape the disease [49]. It reached a sixth edition next year, but I again made the mistake of bringing it out anonymously, so that people going to the tropics, not knowing by whom it was written, omitted to buy it or to take its advice. It seems, however, to have been the first of a long series of similar popular works some of which are deservedly still on the market. Two years later I rewrote and extended the book under another name [66]; and this has been selling ever since, although rather out of date.

CHAPTER VIII

STILL WAITING, 1900

MEANTIME I had been proceeding with the writing of the official report of our recent expedition in Sierra Leone [52]. My colleagues left the task entirely to me, but the report finally appeared in our joint names. It was inscribed to Mr. Joseph Chamberlain, then Secretary of State for the Colonies. At that time the South African War had commenced and Austen, who was in the Artists' Rifles, had gone there, leaving the description of *A. funestus* to Colonel Giles, who therefore gave that mosquito its official name and description. There were eighteen microphotographs of the mosquito-cycle of the *Plasmodia* in the book, drawings of two *Anopheles* made at the British Museum, scenes in Freetown, and my own sketch showing the respective attitudes of *Anopheles* and *Culex* when seated—an important rough method for distinguishing *Anopheles* at a glance. The School Committee also included a drawing by Dr. Fielding-Ould which was not unlike one by G. B. Grassi, which in its turn was far from unlike one of my *Proteosoma* drawings with embellishments [32]. Grassi, who always believed in the excellent tactical device of accusing his accusers of his own fault, now blamed *me* for

plagiarism ! The report also contained some previously unpublished observations of mine, 4 maps, and 2 supplementary reports ; and appeared in February 1900 as a publication of the Liverpool School of Tropical Medicine. It was really the culmination of my work on malaria and mosquitoes, but it attracted little attention in England where everyone was full of the great Italian discoveries made “*indipendentemente da Ross*”—see for instance my *Memoirs*, pp. 392–393.

Dr. Laveran reviewed both my books in the *Bulletin de l'Académie de Médecine* and gave an account of the founding of the Liverpool School (3rd April 1900).

That veteran of science, Sir James Crichton-Browne, F.R.S., who held that position so long as thirty years ago and still holds it, invited me to give one of the famous Friday evening lectures at the Royal Institution. I did so on the 2nd March 1900, with the Duke of Northumberland presiding [53].

On the 28th May 1900 I gave a similar lecture at Cambridge organized by Dr. G. H. F. Nuttall, who was now working there, and dined later with Dr. Shipley at Christ's and with Professor Sims Woodhead.

In November 1899, Professor E. Ray Lankester, then Director of the British Museum of Natural History, South Kensington, London, who had taken me with him to Paris in the Spring in order to introduce me to Laveran and Metchnikof at the Institut Pasteur, asked me for a series of diagrams to illustrate the whole life-cycle of the *Plasmodia* for his *Quarterly Journal of Microscopical Science*. I did the diagrams and wrote a concise description of them, Fielding-Ould made a fair copy of the drawings, and the whole

appeared under our joint names with a note by Ray Lankester in July 1900 [56], almost on the same date as Grassi's *Studi di uno zoologo sulla malaria* [57]. But I hope that our simple statement and humble plates contained nothing but the truth, whereas Grassi's sumptuous letter-press and double-page illustrations cast ours into the shade, but were, certainly as regards the letter-press, very far away from ideal veracity (see p. 36).

Four other works which appeared at about this time ought to be mentioned here. [50] records the valuable observations of R. Koch and gives me full credit, in a single sentence, for my work on malaria and mosquitoes; [54] by Giles, is, I believe, the first English book on all the mosquitoes then known. It was soon followed by [63] by Theobald, a most laborious work on the same subject in six volumes, all of which were kindly sent to me by the British Museum. Of course many new species of mosquitoes have been discovered throughout the world since the dates of Giles's and Theobald's books, and the former contains some serious mistakes as well. On page 153 of his second edition he was kind enough, shortly after he had returned from an illuminating visit to Grassi and his colleagues in Italy, to excuse me for my mistakes. Many years later, when I met Giles in London during the war—he was a friend of mine—I asked him what precisely were the mistakes of mine to which he had alluded, and learnt that he had never read my original papers at all but had judged merely from what he had been informed. I suppose that he had been simply told the old but deliberate falsehood about my

mosquitoes of August 1897—that they may have previously “*punto altro animale.*” That is the way in which text-books are compiled! Nevertheless, Giles’s chapter VIII on the epidemiological relations of malaria and mosquitoes is an excellent piece of work, though it may have been largely derived from my previous papers.

The fourth book which I have referred to, was a translation by J. J. Eyre of the second edition of a book on “Malaria according to the new researches,” by the late Professor Angelo Celli, Director of the Institute of Hygiene at Rome (Longmans Green & Co., 1901). Celli had been part author with Marchiafava of the work on malaria which I have mentioned (p. 11). I met him later at a German Medical Congress, and thought that he really believed most of his statements. He was kind enough to give me the credit of having discovered the mosquito-cycle of malaria which he called “the Ross Cycle”—this was a small mercy for which I am duly thankful in a medical writer, but I was not much impressed with his work. It was an obviously typical medical “*omnium gatherum*” designed to be a kind of feeding bottle for young doctors to give them sufficient food of all kinds ready for assimilation if possible. Although it showed many examples of the *omissio veri* and therefore necessarily of the *suggestio falsi*, I could not find in it any direct *affirmatio falsi*. Evidently under the influence of the falsehood mentioned in the preceding paragraph, Celli does not deign to notice my previous work on human malaria, but being merely a doctor, he does not seem to recognize clearly that the discovery of the life-

history of *P. relictum* covers the discovery of the life-histories of the human parasites in mosquitoes and all similar life-histories. The whole history of the subject is so confused in Celli's book that an ignorant reader is impressed only with the magnificent and original Italian discoveries. In his preface he actually hints, as a British writer in *The Times*, 30th January 1900, had done, that my work in Sierra Leone was plagiarized from his countrymen. The book contains little that is original except perhaps some facts about Roman malaria, and I cannot find a hint in it regarding the chief object of my own studies, viz., malaria-control by mosquito-reduction. What has happened to the book since that date I do not know, but I am still much impressed with the facility with which Italian writings have appeared translated in the British press. At that time, in England at least if not in Italy, I was the plagiarist and the Roman gentlemen were the poor innocent sufferers—that is, the owner was accused of robbing the thieves!

I think it was in the Spring of 1900 that Manson told me of an admirable crucial experiment which he had devised, viz.:

- (1) to infect volunteers in London by the bites of infected mosquitoes brought from Italy; and simultaneously
- (2) to keep healthy persons for some months in a mosquito-proof house in some intensely malarious spot in the Campagna.

The first experiment would certainly provide a most striking confirmation of the mosquito-theory. The four alleged infections of healthy men in Rome in 1898-9 were not altogether above suspicion, because

Rome is surrounded by malarious areas, but (2) would not be scientifically sound unless it were continued for a long period. My reasons for this opinion were that even persons who are obliged to live in intensely malarious spots do not necessarily become infected at once. It will always be a question of chance whether they happen to be bitten by an infected mosquito during the period of trial. By good luck many people escape for months at a time, as proved by many experiences. For instance, during the war many soldiers escaped entirely even in the Salonika valley, though their comrades were often attacked immediately. Chance has always much to do with the result—not every “bullet finds its billet.” Some people are lucky and some are not.

There is another consideration which must be remembered. Mosquitoes often show a preference for particular houses. If a new hut or house is erected anywhere, the insects must either tend to frequent it or to avoid it. Particular houses are generally pointed out in malarious stations where many cases often occur one after the other, while others are reputed to be healthy. It does not follow with certainty that the inmates of an unprotected house are sure to get fever within a few weeks even in the most dangerous localities.

Both of Manson's experiments were entirely successful. A number of *A. maculipennis* were fed on a case of *P. vivax* in Rome from the 17th to 28th August 1900 and were then brought to London and allowed to bite Mr. P. Thorburn Manson on the 29th and 31st August. He began to have fever on the 13th September and

Manson telegraphed for me on the 17th September to witness the result. Unfortunately I could not leave Liverpool to see the patient, but the tertian parasites were duly found in Mr. Manson's blood. A second subject was similarly infected in London later.

A more brilliant demonstration of the mosquito-theory could not have been devised. I am, however, not much in favour of this method of proof. A single event may be disputed afterwards by persons who may allege scepticism as to results, as in modern history where it has even been doubted whether the Spanish Armada ever existed or the Battle of Waterloo was ever fought. A better method of proof is to state not only that an event occurred, but how exactly it occurred. Anyone who doubts my mosquito-cycle of the malaria-parasite is free to repeat that cycle in order to convince himself, but we cannot repeat the Battle of Waterloo for anyone who doubts that it has ever happened.

While Mr. Manson and Mr. Warren were being infected in London, three gentlemen, viz. Drs. Low and Sambon and, I believe, M. Terzi, lived in a hut protected against the entry of mosquitoes at Ostia from the 19th July to some date in November, and remained perfectly free from malaria, though that was the worst time of the year. Would they have certainly become infected anyway during that short period?

In my *Prevention of Malaria* [84] I collected, up to 1911, no less than 32 experimental infections of healthy persons with *Anopheles* carried out by Fearnside, Buchanan, Schüffner and Jancsó in various parts of the world, together with 51 infections by means of

blood taken from infected persons by a number of people. This was all very well ; but much unnecessary suffering must have been produced “ in the cause of science.” In my opinion, there is not much good in piling up academical proofs for proving the proved. Since then many more similar infections, both by mosquitoes and by blood-transference, have been done for the purpose of curing general paralysis by the malarial-parasite and with excellent results, for which the inventor, Dr. J. Wagner-Jaureg, has recently received a Nobel Prize.

During 1900 I also attempted to organize a public conference on the practical prevention of malaria on 25th July 1900. Several foreigners promised to attend, but most of my countrymen were otherwise engaged. They took great interest in the mosquito-malaria theory, but apparently little in the application of it : and the attempt failed. Later in the year I also sent a memorial to the President of the Royal Society (Lord Lister), signed by many distinguished men, begging him to approach departments of the Imperial government with a view to getting them to reduce mosquitoes in the areas dealt with by them. This also remained without result.

All this time the Malaria Commission of the Royal Society, consisting of Dr. C. W. Daniels, Dr. J. W. W. Stephens and Dr. C. R. Christophers, had been studying malaria very ably in British Central Africa and in Calcutta (as I have said). Daniels did extensive work on the habits and breeding places of *A. costalis* and *A. funestus* which were in British Central Africa as in Sierra Leone. Their observations were published by

the Royal Society in eight series of reports from the 6th July 1900 to the 10th October 1903, and Stephens and Christophers recorded valuable observations on malaria and blackwater fever and on mosquitoes in British Central Africa, first in Sierra Leone and then (on my advice) in India.

The world should be duly grateful to the Royal Society for its Malaria Commission, which did a large amount of important and detailed work before it was closed in 1902. In 1900, Daniels had an attack of blackwater fever, came home, and was subsequently made Director of the London School of Tropical Medicine. After the Commission was closed Stephens succeeded Dr. H. E. Annett in the Liverpool School of Tropical Medicine, and Christophers joined the Indian Medical Service, in which he has continued innumerable researches for many years.

After he confirmed my work on the malaria of birds in 1898-9 in Italy and Germany, R. Koch led a German Malaria Expedition to East Africa and New Guinea. Long before this C. W. Daniels and other physicians in British Guinea had noticed that the spleen of native children was much enlarged and blackened by pigment, but that this discolouration disappeared in later life. I had even remarked how frequently the *Plasmodia* occurred in children in India, but it was left to Koch to recognize the very important law which underlies these facts. Most children in malarious places become infected, but when they reach puberty they tend to throw off the infection without treatment. That is, the parasites die out in them. In technical language they acquire immunity against malaria, just

as people acquire immunity against scarlet fever, measles, and smallpox. Koch showed that in malarious places it is the native children who are "the reservoirs of infection"; newly arrived Europeans become infected from them through the mosquitoes, whilst native adults, who have already survived the illness, are comparatively immune—a most important generalization which was quickly confirmed by the Malaria Commission of the Royal Society and by our own Expeditions to West Africa.

Koch also suggested a new method of preventing malaria by removing the malaria-parasites from all cases in a locality by thorough quinine-treatment. He tried the method at Stephansort, New Guinea, in 1900, with rapid and good results, and it was soon followed up in other German possessions (see Professor Schilling's contribution to my book [91]). It is often useful where mosquito-control would be too expensive; but the method costs money also. A summary of the German work was put in the *Deut. Med. Wochenschr.*, 1900, No. 49.

CHAPTER IX

AN AMERICAN DISCOVERY

FOR centuries past yellow fever had been the curse of the new continent: scarcely anyone could reside long in tropical America without contracting it, and as the mortality was about 25 *per cent.*, this fact was a great bar to the development of those parts, which had to be given up almost entirely to the indigenous population.

Numerous attempts to find the causative agent of yellow fever have been made; but they have all failed up to the present. But even when the causative agent of any disease cannot be precisely ascertained, we can often do much useful work by determining the method by which the disease is carried from man to man. For a long time past various observers have surmised that yellow fever may be communicated by mosquito-bites, and Dr. Charles Finlay, of Havana, in 1881, repeated this hypothesis and even claimed to have communicated the disease experimentally by the insects' bites. But there must have been some mistake somewhere because it was proved later that the poison, whatever it is, must be incubated in the mosquitoes for about 12 days before it can be communicated to a healthy person, whereas Findlay seemed

to think that the poison was taken from patients to healthy persons directly. It is easy for persons to sit in armchairs and weave hypotheses ; many imagined America before Columbus ; but an ocean had to be traversed between the dreams and the reality. Theorists who do not trouble to verify their own speculations deserve little credit ; but C. Findlay was of a better order. Living, as he did, in the centre of yellow fever, he was able to form some conjecture as to the species of mosquitoes involved—a pot-breeding, brindled mosquito, then called *Stegomyia fasciata* or *calopus*, now called *Aedes aegypti*, very common in Havana and indeed in most tropical countries.

Early in 1900 yellow fever appeared among the American troops in Havana. Surgeon - General Sternberg, who was a straight thinker, immediately appointed a Commission to investigate it, consisting of Dr. Walter Reed, Dr. James Carroll, Dr. Jesse W. Lazear and Dr. Aristides Agramonte. They commenced work at Havana in July 1900, by speedily disproving some speculations as to the cause of the disease. They next determined to investigate by direct experiments the carrying capacities of mosquitoes on the basis of my work on malaria, according to which the virus must develop for some days within the carrying mosquito and could not be communicated directly from man to man. In fact that most sagacious observer—Dr. H. R. Carter—had actually proved some time previously that an interval of about 12 days must elapse between the arrival of a case in a locality and the infection of a secondary case.

But the Commission actually began work with

Finlay's mosquitoes hatched from eggs which he gave them for the purpose. On the 27th August 1900, Carroll allowed himself to be bitten by an *Aedes* which had been fed 12 days previously on a severe case of yellow fever. He was taken ill on 31st August with typical yellow fever and nearly died. A second similar case X.Y. also became infected ; but nine men who had been bitten by this species of mosquito which had not been kept for 12 days after their original feeds, remained well. While Carroll was ill, Lazear, who happened to be working in the yellow fever ward, was accidentally bitten in the hand. If I remember rightly, Dr. Lyster told me at Panama in 1904 that he was present and that Lazear remarked, "I wonder whether this creature is infected," but allowed it to go on feeding. Lazear was taken ill a few days later and died on the 18th September.

But the Commission was not yet satisfied and built a Camp in an isolated spot called Camp Lazear, at which stringent crucial experiments were conducted. It contained two frame buildings, each 14 × 20 feet in size. In one of these, plucky young American soldiers who volunteered for the purpose were kept for twenty nights exposed to the horrible soiled clothing and bedding of yellow fever cases—without a single one of them becoming infected. In the other building Private J. R. Kissinger, who said he volunteered solely in the interests of humanity and the cause of Science, was bitten by five mosquitoes on the 5th December and became ill on the 8th December. Next week three of the American soldiers who had previously been exposed to the soiled clothing were

now bitten by infected *Aëdes* and were subsequently attacked with yellow fever. Lastly, John P. Moran was bitten in the chamber on the 21st December, and was taken ill on Christmas morning. On the last day of the dying century Reed, the leader of the Commission, wrote home to his wife: “. . . The prayer that has been mine for twenty years, that I might be permitted in some way or at some time to do something to alleviate human suffering, has been granted.”

Reed died rather suddenly on the 22nd November 1902, actually in a state of apprehension regarding the future of his wife and daughter. The wealthy American people had allowed him to die without any adequate honours or reward. This was not only a case of ingratitude but one of impolicy. If the world refuses to pay for world-service while it allows anyone to enrich himself by self-service, it is the world that ultimately suffers for its own folly.

CHAPTER X

A DASH FOR VICTORY

LIKE the British, the Americans have always tended to refuse any monetary payment for medical discoveries however important. So far as I can ascertain, none of the men who discovered how yellow fever is carried was given any reward.

Most of them are now dead. There is some absurd idea that medical discovery should be gratuitous—why, I cannot imagine. The yellow fever discovery has probably done more for tropical America than any other work and must have greatly improved that part of the world. Surely the best way to encourage similar discoveries in the future would be to render some award of some kind at least to those who effect them.

But unlike the British, the Americans have shown a brisk desire to profit from the work of their unpaid benefactors, and only a few weeks elapsed after yellow fever was shown to be carried by mosquitoes when the Americans commenced attempts to reduce the disease by the simple process of reducing the carriers—a process quite beyond the British intellect. Major and Surgeon M. C. Gorgas, U.S.A., the chief Sanitary Officer of Havana, Cuba, commenced mosquito-control

there in February 1901, and although the city of Havana is a large one, he informed me on the 19th November 1901, that mosquitoes had been much decreased there, while no deaths from yellow fever at all had occurred in that year, though there had been 74 deaths from yellow fever during the previous year, while even the malaria had been diminished. Gorgas thinks in that letter (see my *Memoirs*, p. 453) that the disappearance of yellow fever was almost altogether due to killing infected mosquitoes at the infected points by burning pyrethrum powder in the infected house and all the neighbouring houses.

But nothing like similar promptitude could be observed in British possessions against malaria. So far as I could learn, my discovery had been allowed to remain without practical results throughout the British Empire. It is true that in Calcutta, the municipality had continued my assistant, Mohamed Bux, on the magnificent salary of 16 rupees a month, apparently for the purpose of keeping that great city free of the insects. Similarly after our visit to Freetown in 1899, the Corporation had kindly determined to follow my advice as regards mosquito-control there, by appointing a native on a salary of one pound a month, in order to cure the White Man's Grave of its evil reputation. Needless to say malaria continued both in Calcutta and in Freetown in spite of such munificence, which was speedily stopped.

On the other hand, pricked by their tender sanitary consciences, they generously determined to give £500 a year to one of the medical officers of Freetown to do duty also as the health officer of that city; but

as he appeared to be allowed no additional funds for an adequate sanitary labour force, the result seemed to have been very small, for certainly the town was in a truly deplorable condition when we returned in 1901. For this work he was finally and suitably rewarded.

In India also very little seemed to have been attempted since I left the country. My letter to the Director-General [45] appears to have met with absolutely no response in that Empire, except a letter from a medical officer in the same periodical, *The Indian Medical Gazette*, for November 1899, in which he said that "dapple-winged mosquitoes" were to be found in rice-fields round his bungalow in Travancore. In some way he seemed to think that this discovery invalidated my method of mosquito-control, and I heard subsequently that the Indian Sanitary Authorities, always slow to move, had seized upon his welcome paper as a further pretext for doing nothing.

My friend, Colonel W. G. King, I.M.S., Sanitary Commissioner for Madras, seems to have been the first to have taken any practical action against malaria in India. On the 31st May 1900, he wrote to me: "I have started all municipalities on anopheles hunting expeditions." Also two medical officers attempted some mosquito-control work in Calcutta (*Indian Medical Gazette*, Nov. 1900, and *Lancet*, 8th Sept. 1900). On no less than two occasions they put tar into a ditch which was causing malaria in some coolie lines, but seemed to be much disappointed that the fever did not immediately vanish. By that time

my ideas had already become quite discredited in India : for some reason I appeared not to be popular there, and was accused of holding many foolish opinions—so much so that I was forced to protest as follows (Ind. Med. Gaz., Nov. 1900) :

“ The habit of imputing to a writer opinions which he has never expressed and has indeed often disclaimed, and of then demonstrating simultaneously the folly of these opinions and of the writer for holding them, is one to be guarded against. I have really never expressed the ‘ingenious suggestions,’ which Captain . . . seems to think I have regarding the possibility of exterminating anopheles from, let us say, the whole of Bengal ! The utmost I ventured to suggest was that it might be possible to exterminate them from some large towns, cantonments, and plantations *under favourable* conditions. So I think it is : but I have always expressly excluded large rural areas from this suggestion. The idea that vast tracts, peopled only with natives, can be freed from any mosquitoes is too silly even to require a disclaimer.”

The same absurdities are still often imputed to me. Neither then nor for years afterwards could I find in Indian writings any intelligent understanding of my proposals, much less any serious efforts to apply them.

Similarly in Sierra Leone. In my report to the Governor I had advised mosquito-control by drainage, by oil, and by hand ; wire-gauze to windows, especially of hospitals and other public buildings ; bed-nets for patients and, if possible, for everyone ; and better houses and segregation for Europeans.

From accounts which were given to us in Liverpool by our later expeditions, by clerks, and by others returning from Sierra Leone, we learnt that absolutely

nothing was being done there during 1900 in the practical sanitary line, but as I said in Chapter VIII, Drs. Stephens and Christophers had been sent there by the Royal Society to continue investigations on the mosquitoes. They had done much admirable work, but not being experienced sanitary experts I think that they tended to retard practical amelioration by somewhat exaggerating its difficulties. Thus they laboriously proved by experiments that fresh crops of larvae appeared in pools in which previous crops had been destroyed by oiling—as if anyone had ever dreamed that anything else was likely to occur.

Doctors to the right of me, doctors to the left of me, laughed at the mere notion of reducing mosquitoes. Some even said it was wicked: even my colleague, Boyce, thought I had gone too far, and Mr. Jones, our Chairman, changed the subject when I mentioned it. Sir Michael Foster asked my advice regarding the prevention of malaria for use in a pamphlet which he was preparing, but when it appeared my suggestions regarding mosquito-control were omitted.

At the same time while these suggestions were evidently not worth consideration, all kinds of ingenious devices were recommended by all kinds of people, some of whom had never been to the tropics. The most popular methods (in England) were to leave the mosquitoes to swarm everywhere as before, but (1) to give quinine to everyone as suggested by Koch (p. 69) or (2) to protect all dwellings by wire-gauze. Imagine these measures applied to a large town like Freetown containing 30,000 negroes, including some thousands of infected children and many large and

pragmatic mothers. It would require a regiment of doctors and dispensers to administer the quinine, a regiment of soldiers to keep order, and a regiment of newspaper editors at home to overcome the political consequences which would follow—imagine the squalling of the children, the fury of the mothers, the angry silence of the fathers, and the lofty protests of the politicians. Then to protect all the houses—wooden thatched shanties with holes and chinks everywhere in floors, walls, and roofs—we would have to convert each into a kind of meat-safe, costing much more than the original dwelling—a city of meat-safes! At the same time, out in the open, people were to wear veils, gloves, and “mosquito-boots” in the hottest weather (especially if they were soldiers marching to battle!)—though in many places in the tropics mosquitoes seldom bite in the open. Other experts recommended everyone to keep his skin smeared with “culicifuges,” that is, lotions or ointments supposed to repel the insects. But a few did support mosquito-reduction—only not by draining or oiling breeding-waters, as I recommended. One person invented a lamp to attract and destroy thousands of the adult mosquitoes every night—a most ingenious invention, though unfortunately mosquitoes are not attracted by light. Others said that a loud humming note of a certain pitch would draw in all the male mosquitoes from far and wide, and a certain great inventor set to work to devise a suitable machine for this purpose (there may be something in this). Others again were convinced that certain plants, if kept in bedrooms and verandahs, would repel the enemies of mankind—and so on.

On the other hand, mosquito-reduction, though it is really only a refinement of the very ancient and well-approved method of banishing malaria by drainage (and is indeed applied every year at certain seasons by Nature herself), was derided by most of the scientists.

It is worth asking why? There is the scientist with eyes to see but no brain to think, and the scientist with brain to think but no eyes to see: and the combination is rare. The biologist often does not possess the calculative faculty—which Plato rightly decreed was essential to those who were to be admitted into his Academy. The doctor and the zoologist are trained rather in observation than in calculation; the one thinks in terms of medicine and the other in terms of classification and bionomics. The doctor is often entirely ignorant of practical sanitation; he stands aghast before a few roadside puddles and the problems of town-management, but delights in the idea of pouring quinine down everyone's throat for ever—especially if, as happens in certain foreign countries, he dispenses his own cures. The entomologist is busy over wing-veins and the pathologist over parasites; and application to life-saving is apt to be of secondary interest to them. On the other hand, this is the sanitarian's principal motive, and he must be made up of calculation; he deals with men in the mass; he fits his measures to his means; and his great science of epidemiology should be largely a branch of applied mathematics.

As to the possibility of reducing mosquitoes, that was self-evident. They are not uniformly diffused

like a gas, but vary greatly in numbers from spot to spot. For all we know each may be able to fly a hundred miles during its life ; but actually they tend, like other animals, to congregate where the conditions are most favourable.* It is for us to render the conditions unfavourable. The amount of malaria in a place must be a function of the number of carrying *Anopheles* there, and we can at least reduce both if not banish both. *Ceteris paribus*, the *per capita* cost of this must vary inversely as the density of the human population—that is, be least in towns. Mosquitoes are vermin. To reduce vermin one destroys them and their breeding places as fast as possible. One does not stand about and think over it.

This, however, is exactly what the British had been doing for years. It was no use arguing with them because few of the glibbest talkers ever read what one wrote, or even knew the meaning of the mathematical word *function*. But, as I learnt subsequently, there were other reasons. One gets much more *kudos* by writing scientific papers than by doing humdrum sanitary work, however useful ; and governments prefer spending money on more visible things, such as new buildings, additional secretaries, and hinterland campaigns.

Early in March 1901 our School in Liverpool was asked to discuss the matter of malaria-control with the Chambers of Commerce at Manchester. I drew up a programme of the simple sanitary reforms required for West Africa.

It was decided to forward these recommendations to the Colonial Office and to ask Mr. Chamberlain to

* See pp. 155, 168.

receive a deputation on the subject. He agreed and saw us on March 15th, 1901.

This was my first, but by no means my last, experience of such events. They remind me of a clever picture which I possess by Mr. Gordon Browne, R.I., called "A Deputation to Circe." The poor sailors to whom that beautiful but cruel enchantress has given the grotesque heads of various animals stand trembling before her on the sea-sand of her island, evidently beseeching her to restore them, while she sits aloof and aloft upon a rock laughing at them ! So with modern deputations : most of the brief time allotted is wasted in preliminary complimentary speeches : those who know the business are then allowed a few minutes each ; and lastly, the Minister (who is always on his defence) easily scatters the experts with a few Pythian shafts feathered with the *argumentum ad ignorantiam* : and the deputation files out again, glad to have any heads at all left on their shoulders. I think on that fateful morning some of us breakfasted with Alfred Jones in a club in Northumberland Avenue. Next we had a preliminary meeting to discuss our procedure, at which we were joined by Mr. F. Swanzy, who represented the London Chamber of Commerce. Mr. W. F. Lawrence, M.P., introduced us ; and there, in a large dark room in Whitehall, was the great little Joseph Chamberlain, seated before us with his political trade-marks, the orchid and eyeglass complete. He was accompanied by some of his officials, I have forgotten whom (Manson was not present) ; and after the introduction, Jones, who was then, I think, President of the Liverpool Chamber, followed with a speech designed

to assuage in anticipation the Minister's wrath by quoting some of the Minister's own words. The Manchester representative then wasted much time over a local sanitary defect near his business place in Lagos ; good Mr. Swanzy spoke briefly for London ; our Dean, Boyce, reiterated our recommendations ; and Dr. Carter, of the Southern Hospital at Liverpool, wasted more time. Next came the turn of us three scientific helots, whose combined salaries scarcely amounted to £600 a year ; someone looked at his watch and the reporters yawned. I compared West African sanitary organization with that at home and in India, and insisted upon what every sanitarian of the smallest experience knows to be the fundamental necessities—sanitary commissioners to inspect, advise, and report—like those employed in India, and like the Inspectors of the Ministry of Health in England—without whom local measures are apt to degenerate into zero. Fielding-Ould gave some of his actual experiences ; and Annett said, regarding Freetown : “ I must state that on my second visit to that town last year it was with extreme disappointment that I perceived no indications whatever that the slightest effort was being made to improve the wretched conditions there, either in the direction of suggestions of the Expedition or in any other way.” Regarding Nigeria he added : “ I urge that in these Nigerian stations, with their small European communities, a few inexpensive measures, intelligently executed now, would render those stations absolutely free from malaria.” Then Chamberlain rose and demolished us—while the reporters scribbled as fast as they could. He rejected

our proposal regarding sanitary commissioners, by whom he seemed to think we meant backyard sanitary inspectors ! “ If,” he said, “ we are to have anything equivalent to a house-to-house inspection in these colonies in the West Coast of natives as well as of Europeans ; if the inspectors in turn are to be inspected by a superior chief, or head, with all the scientific requirements which are necessary ; and if, again, these inspectors are to be supervised by travelling inspectors and commissioners sent from this country—I confess I tremble at the budget which will be produced.” Neither he nor his officials had understood in the least what we meant. As usual with politicians, he deprecated expenditure, not recognizing that sanitary expenditure is an insurance against the much greater expenditure caused by sickness, as that on fire-engines is against fire. On the other hand, he was “ prepared to consider ” a travelling commission of three business men and one scientific expert, all of whom would have to be paid by the Chambers of Commerce for doing the business of the Colonial Office. This proposal was characteristic of British administration : instead of doing cheap and necessary work it spends large sums on expensive and worthless talk. The proceedings now closed with more compliments. Chamberlain had done some good (and won much political capital) by suggesting the schools of tropical medicine : but in my opinion, his refusal of a proper sanitary organization for the colonies largely cancelled, then and since, the benefits which might have accrued. I suppose I was the only one present who had any real knowledge of tropical sanita-

tion ; and I remember thinking to myself angrily as I left stately Whitehall : " These people are no longer fit to hold the hegemony of the world." Probably the fault lay with the permanent officials : but in either case my dreams of general British action against malaria vanished at that interview. Jones and Boyce attributed everything to jealousy.

Of course the proposed commission came to nothing. Business men were not likely to undertake such a job. I suppose Mr. Chamberlain thought I had suggested a sanitary commissioner for West Africa in order to find a job for myself. At that time I could have returned to India with a large salary whenever I pleased. Neither appointment was possible to me, because my capacity for continued service in the tropics was exhausted.

But I was ready to pay short visits there ; and now fortune favoured us again. Something I had said about my project bore fruit, and early in April I received an enquiry from Mr. James Coats, junior, of Glasgow, regarding my schemes.

Early in 1901, before I knew anything of the yellow fever work, I had determined on a temerarious and Quixotic action. I proposed, in short, to go to Freetown with an assistant, to hire labourers there, to buy pickaxes and shovels, to show them how to reduce mosquitoes or to die in the attempt.

It was really an unheard-of piece of audacity—I was going there to take over the unused functions of the British Government. It was sanitary rebellion ; sanitary Bolshevism. I proposed to supersede His Majesty's lawfully constituted Sanitary Department

of Sierra Leone ; to “ wipe the eye ” of the Governor and Council ; to kill his mosquitoes under the very nose of the newly appointed Health Officer, with his additional salary of £500 a year and no staff ! *And we did it.*

While I was mooting this wild project to men of business in Liverpool who were genuinely interested in West Africa, news came (Lancet, 12th January 1901) that Dr. J. C. Thomson, of the Medical Department of Hongkong, had commenced investigating mosquitoes there with a view to their reduction ; and I also heard from Dr. H. Strachan, of Lagos, that Sir William MacGregor, the Governor, was proposing to drain large areas of marsh and to enforce general quinine prophylaxis among the Europeans in that town. The yellow fever news must have reached us about March but the work of Gorgas could scarcely have become known to us until some months later.

My pleasure may be imagined when Mr. Coats, in a letter of 30th April 1901, offered to give £1,000 to pay the expenses of the mosquito-control scheme which I was contemplating. He thought that the sum should permit of a year's trial of my plan, and just before we left England in consequence, he doubled the amount.

CHAPTER XI

SIR WILLIAM MACGREGOR, 1901

I SPREAD the good news everywhere and at once prepared for the expedition, which I decided should reach Sierra Leone in June, before the onset of the rains and of the malaria-season. This left me only six weeks to make all arrangements. My first care was to find a reliable assistant, whom I could leave to carry on the work on the Coast after my return to my teaching duties in Liverpool. The post was advertised; and I finally selected, among several candidates, a young Scotsman, Mr. Logan Taylor, M.B., B.S., of the pathological laboratory of Glasgow University, whose ability and devotion to work cannot be too highly praised; his salary was to be at the rate of £500 a year. In order to obtain complete freedom of action I determined to keep Mr. Coats's money under my own control to begin with and to hand it over to the School after my return; and for this purpose I opened an account, which I called later the Tropical Sanitation Fund, at the Bank of British West Africa. Of course I could not take anything out of this for myself, either for salary, allowance, outfit, or even expenses (except cost of a passage from Lagos to Accra); and my poor pocket was again

further depleted very considerably in consequence. But Mr. Coats's generosity enabled me to enlarge our scope, and I determined to extend my own visit to the Gambia, the Gold Coast, and Lagos. Additional help came from friends—Mr. Max Muspratt, Mr. F. Swanzy and Mr. John Holt—who gave us barrels of cement for filling up the mosquito-pools, barrels of crude petroleum and creosote, and pickaxes, shovels, and watering-pots for spraying oil. Mr. Alfred Jones gave us free passages anywhere in his steamers. Surgeon-General Harvey, Director-General, Indian Medical Service, loaned to me one of his officers—Lt. A. G. McKendrick, I.M.S., to learn the work. On the other hand, the Colonial Office refused to do the like with Dr. G. Williamson, one of its medical officers in Cyprus, who wished to take a course under me at Liverpool, because they said, "so far as we can make out, Dr. Williamson has had no experience of malaria, as the disease is practically non-existent in Cyprus." They had only to look at their own reports to ascertain the contrary, and in 1913 they were obliged to send me to Cyprus to deal with the malaria there. Countries conducted in this manner can scarcely prosper.

But Mr. Chamberlain agreed to our adventure, and even gave us his blessing. I was careful to explain to everyone before we started that we proposed *only to give an object-lesson in mosquito-reduction*; we could not continue the work indefinitely nor did we hope to banish malaria from the whole of Africa for the sum of one thousand pounds. Yet this is precisely what we were afterwards accused by the West African residents, especially by the doctors, of attempting.

Although I did not know it till later, mosquito-control was being commenced at about the same time by Drs. W. H. Berkeley and A. H. Doty near New York, and by Dr. Malcolm Watson in the Federated Malay States—the beginning of a magnificent campaign [91, 99]. There had also been an interesting outbreak of malaria in Holland, dealt with by turning the sea into the canals (*British Medical Journal*, 26th January 1901).

Before leaving England I received two honours. I was made a Fellow of the Royal College of Surgeons without examination, as I had been a Member for twenty years (11th April); and I was elected a Fellow of the Royal Society (7th June). Mr. Alfred Jones gave us a grand farewell banquet (6th June), which Surgeon-General Harvey attended all the way from London; and we started for Sierra Leone on the 15th June 1901.

We reached Bathurst, the capital of British Gambia, on the 28th June—a small town on an island, or rather a sandbank in the middle of the river. As I expected, the people had done absolutely nothing against their mosquitoes, but the Governor, Sir George Denton, agreed to receive someone from our School later to commence the work.

We arrived at Freetown on the 2nd July 1901; and stayed with the Governor there, Sir Charles King-Harman, until we could hire a house for ourselves. To my surprise, there were not nearly so many mosquitoes at Government House as in 1899 because, we heard, Captain Hodgins, the Governor's A.D.C., had cleared out the pot-breeding mosquitoes before our

visit ; but I found the breeding-pools of *Anopheles* in the town just as we had left them in September 1899. Holes, puddles, and ditches, which could have been drained or filled up at the cost of a few shillings each, were still swarming with larvae exactly as before. I thought that Freetown was not only the White Man's grave, but the grave of his reputation also.

Logan Taylor commenced work without delay and immediately engaged the services of over 20 men under intelligent headmen : to these the Governor added 12 men and the necessary carts and implements : I decided to adopt what I now call *general* mosquito-control and not only *special* control against *Anopheles*. Our labour force was therefore divided into two gangs : a small gang of 6 men (called the *Culex* gang), to collect from private houses all the broken bottles and pots, empty tins, old calabashes and similar unconsidered vessels, in which *Aedes* and *Culex* breed ; and a larger gang, called the *Anopheles* gang, to drain or to fill up the puddles in the streets and backyards in which *Anopheles* breed.

The *Culex* gang did very rapid work. They piled the rubbish into carts which they discharged into an assigned rubbish shoot. At the same time they showed the larvae to the occupants of the houses and instructed them in the manner of destroying them by emptying out the vessels which contained them, or by dropping a little oil on the surface of the water in which they live. It was found that on the average this gang cleared about fifty houses and removed about ten cartloads of empty tins and broken bottles daily.

The occupants welcomed the gang wherever it went, and some stated that this had not been done for years. The effect on the prevalence of *Aedes* and *Culex* can be imagined when I remark that about one-third of the tins and bottles contained the larvae at this season [64]. Every house had been previously breeding its mosquitoes in its own backyard or garden.

The *Anopheles* gang had a more difficult task. The breeding pools of these insects in Freetown, both during the rainy and dry weather, had been minutely described by two previous scientific expeditions. At this season the water-courses contained impetuous torrents too rapid for larvae to live in; but the streets, yards, and gardens possessed numerous pools of rain-water, well suited for them. These were attacked by many methods. Some were filled with earth, rubble, and turf. Others were evacuated by cutting through the rock which contained them, or by making channels in the soft earth. Owing to the large rainfall (estimated at about 160 inches annually), to the peculiar nature of the ground, and to the very defective surface-drains, these puddles were exceptionally numerous in Freetown; and in order to drain or fill up many of them as soon as possible it was deemed advisable to adopt the simplest and least expensive methods at first and to reserve more permanent works for the future. At the same time several men were specially employed in brushing out the puddles with brooms, or in treating them with crude petroleum or creosote. Progress was fairly rapid in spite of the deluge of rain; and many of the worst streets were fairly well drained in a few weeks.

On the 18th July I gave a lecture at which the Governor presided. The Hall was crammed with people of all colours who were appreciative but hilarious. For some reason a photographic lantern slide of a mosquito's stomach excited Homeric laughter, but a resolution supporting us was passed *nemine contradicente*; whereupon the Governor said he would add more men to our labour force.

On the 20th July, leaving Logan Taylor hard at work, I started for Lagos. We had many passengers who kept their spirits up by pouring spirits down, and one of them acquired a black eye in the process. We reached Lagos 26th July. The Governor sent his steam-yacht with his A.D.C. and a letter saying "your room is ready." We traversed the bar and the lagoon and were met by Sir William MacGregor on the steps of his palatial house—different indeed from Freetown. He wore a white pith-helmet, his ribbons and a kilt and sporran of the MacGregor tartan! Dr. Strachan and several of his officers stood behind him. It was a State-reception!

Of all the men I have met I honour him perhaps the most. His father was a farm-labourer at Towie, Aberdeenshire, and he was born in 1846. Assisted by friends and his own assiduity, he studied medicine at the Andersonian College in Glasgow, graduated at Aberdeen University, took his M.D. in 1874, became Assistant Medical Officer in the Seychelles in 1873, and in Mauritius in 1874, and Chief Medical Officer of Fiji a little later. Very soon, however, he was selected for administrative work, and represented Fiji at the first session of the Federal Council of

Australasia in 1885. He was given the Albert Medal and a Medal of the Royal Humane Society of Australasia for saving life during the wreck of a ship. In 1888 he was made the first Administrator of British New Guinea, where he pacified turbulent tribes, explored large new areas, and won the admiration of all. He became Governor of Lagos in 1899.

After tea he showed me how Government House was protected by screens of copper-wire gauze to the windows ; and called up a little negro boy in a smart livery whose sole duty it was to kill any mosquitoes which might enter in spite of the screens.

Then still dressed in topi and kilt and attended by interpreters and others Sir William led me afoot into a neighbouring market where many large and loquacious ladies, amply clad in yellow gowns, were seated at stalls selling fruit, yams, and dried rats ; bowing and taking off his topi to several of these, he enquired about their health (which was quite evidently good) and the health of their children and husbands, and the presence of fever in their homes ; and introduced me to them. After dinner, we examined a number of regulations regarding malaria which he had recently issued. He ordered all his officers to take quinine regularly, and to set an example he nearly poisoned himself with 30 grs. of quinine twice a week and wanted to persuade me to do likewise. I excused myself by pleading the possession of a palm-leaf fan with which I always keep off hungry visitors in the tropics.

Lagos was a much richer city than Freetown and possessed some fine houses, many of which were protected by wire-gauze and lit by electricity, but as

they often had for neighbours bad native slums, while the whole town was on the flat and was surrounded by lagoons and marshes and the rainfall was very heavy, the proposition before us was not an easy one.

Sir William MacGregor was draining or filling up some of the marshes and making embankments and outlets at considerable cost, but he and Strachan had not paid sufficient attention to numerous shallow rain-pools in the sandy streets close to the houses. I found swarms of *A. costalis* in these.

According to a law first stated by me in my Parke's Memorial Prize Essay which won a gold medal and some money in 1895, malaria should vary like light and gravity inversely as the square of the distance from its origin, so that a square yard of puddle at a distance of one yard should be as dangerous as a square mile of marsh one mile distant, while the cost of draining the latter might possibly be 1760 square or 3,097,600 times larger. I should therefore have begun with the puddles, and should have left the marshes for subsequent action if really necessary.

Another scheme of his had been to found a Lagos Ladies League for the destruction of mosquitoes in their houses, and he gave them a grand luncheon on 2nd August, while I was there. I saw no loss of appetite among the ladies either for viands or champagne, and the proceedings were extremely cheerful, our speeches being uproariously cheered by the more sunburnt sisters. But whether the mosquitoes suffered much in consequence I doubt—though the gardens or backyards of some members were certainly well kept. He was also passing legislation against the breeding

of mosquitoes in private premises—as the Americans had done. All such legislation can do no harm ; but in practice the cost of the numerous summonses required to enforce it is apt to exceed the cost of doing the work by departmental agency—as experienced health-officers well know. Municipalities favour the idea because it appears at first sight to put them to no expense ; but we generally observe two things about municipalities—the excellence of their sanitary by-laws and the completeness with which the public ignores them. Which is the cheaper in the end, (a) to make one inspection and then to do the work, or (b) to make many inspections, worry the householder, issue several summonses, be finally forced to do the work, and then try to recover the cost in a law-court ?

He took me for a two-days' trip in his flat-bottomed stern-wheel Government paddle-boat up one of the lagoons to Badagry. It had a trick of veering suddenly to the right and trying to run up the bank ; and it did so on this occasion. Fortunately it did not “stick” for long ; and I was able to enjoy the cool voyage, the luxuriant tropical vegetation, the huge gorgeous butterflies which came on board, and the store of knowledge of such matters possessed by my host.

He was somewhat indignant regarding some of the things, such as this yacht, which the home Government had sent him. They had also supplied him with a locomotive for draining the swamp, which, when it arrived, was found not to fit the rails which had already been laid down at considerable expense. Dr.

Strachan also took me a long day's railway journey (120 miles), through forests and past hills, to the great native city of Ibadan. We spent the whole of next day in traversing this immense collection of rambling mud-houses in our "hammocks," carried by negro bearers. The city seemed to be occupied only by legions of women and children, and when I asked where the men were, I was told they were asleep in their houses! We visited another "government failure," a luxurious and expensive iron house which was so unwisely constructed that it became unbearably hot in the daytime, so that the occupants were obliged to go and sit in the shade of trees outside. We slept in a well-made railway house which was said to be very malarious. Not a single mosquito was to be seen; but Strachan swore that numbers entered in the dead of night. I noticed none because I slept in a good bed-net; but to test Strachan's theory I put one of our servants for the night into an old net with holes in it. Next morning, sure enough, there were five or six gorged *Anopheles* within his net. If we had slept unprotected we might have been bitten by scores of them. This makes a good kind of mosquito-trap for testing the real frequency of *Anopheles* anywhere.

I left Lagos on 5th August 1901, in a pleasant German steamer for Accra. There were a number of officers from German West Africa who talked philosophy to me in English and drank beer to me in German: the military doctors knew all about the work of Koch and myself, which was more than most English doctors did. In about 2 days we arrived at

Accra, where I stayed for 3 days with the Governor—then Major Sir Matthew Nathan, R.E., in order to arrange for sending him someone to start anti-mosquito work there. This part of the coast is much more dry than Freetown or Lagos, but is still very malarious owing to the breeding of *A. costalis* in the borrow-pits, or holes in the ground from which clay is taken to build native houses.

I now went on to Freetown again and reached it on the 16th August. I stayed with the Governor, and after I had examined the work done by our men, he asked me to consider with him his project for making houses for his officials and other Europeans on the hills (500 ft.) near Wilberforce.

I had always wondered why on earth this had not been done years ago, as it would have been done in India. The reasons were that the merchants liked their agents and clerks to live close to the natives, who provided their business. Unfortunately the natives provided their diseases as well as their business. There was also an absurd notion about that it was not proper for white men to segregate themselves from their poor coloured brothers. Of course the latter, who had already become partially immune, were the reservoirs of the malarial infection, conveyed from them to the white men who were not immune. Sentimental arguments like this have always convinced me that human beings are not quite as intellectual as they think.

McKendrick and I were back in England on the 2nd September 1901. I remember nothing of these voyages except that there was an obnoxious journalist

on board one of the ships who insisted on disproving the mosquito-theory to me, and, when I closed the discussion from very weariness, threatened to "expose" me in the press—which he actually did in the *Standard* a little later.

I wished to procure an independent opinion upon the results of our work, and had therefore asked Dr. C. W. Daniels, then Superintendent of the London School, to visit Freetown. He arrived shortly after I left for home, remained some weeks, studied Logan Taylor's work with great care and reported to me after his return to England, as follows :

1st October 1901.

"DEAR ROSS,

I have carefully examined all the various works which have been undertaken with a view to the serious diminution in the number of mosquitoes in Freetown, Sierra Leone. . . .

In my opinion already your efforts have been crowned with a large degree of success. . . .

The operations having been only recently begun, are, of course, as yet far from complete. A considerable part of the town, perhaps half, has not been touched. Even in the parts longest under treatment, in the yards adjoining the streets, there are still numerous breeding grounds ; and in the streets themselves occasional places have either been overlooked or the works undertaken have not been effective as yet. . . .

Though I consider that you have already proved the practicability of exterminating *Anopheles* in Sierra Leone during the wet season, the work is at present incomplete, even in the streets in which most work has been done. . . .

In conclusion, I wish to express my thanks to you personally and to the Liverpool School of Tropical Medicine,

for the opportunity afforded me of seeing the first real British practical application of the principles you have elucidated.

I am,

Yours very sincerely,

C. W. DANIELS, M.B.,

London School of Tropical Medicine."

The full letter is given in [64]. From some passages in this letter I gathered that not even Daniels quite grasped my intention in commencing this work. We were not making an experiment but giving an object-lesson, and therefore I added the following footnote to his letter :

"In order to guard against misapprehensions, it is advisable to state here that we are not now undertaking to prove over again that mosquitoes carry malaria. This fact was fully established long ago. Our present intention is simply to give an object-lesson in the manner of ridding tropical towns of mosquitoes by drainage and cleaning up. We are prepared to spend a large sum of money for this purpose ; but we are not prepared to continue the work for ever. The work—especially the drainage and collection of rubbish—properly belongs to the local authorities. If they choose to continue our efforts, then we can confidently promise that the mosquito-borne disease in Freetown will be, ultimately, very materially reduced. If, however, they discontinue them—if they allow the town to sink back into the condition it was in when we arrived—then I can only say that the mosquito-borne disease will remain. It is for them to choose. I may add, however, that I have no doubt that the former course will be the one adopted.

R. Ross."

Manson, too, did not understand the matter in the least, for he wrote to me on 14th October 1901 :

“ Daniels believes in your mosquito campaign but is *very very* strong on not risking failure and on *concentration* in our plan. This case (?) good and the British Public will do the rest ; this a failure and good-bye for years to mosquito sanitation. *Verb. sap.*” Both of them (and many others) seemed to think that I was trying to prove the possibility of banishing mosquitoes from Freetown *for ever and ever at a single blow*.

It will be seen later what the “ British Public ” actually did do (Chapters XII, XIV).

Accounts of our work will also be found in the British Medical Journal, 7th and 14th September 1901.

As this and the simultaneous campaign of Gorgas at Havana were the first of the kind attempted, it may be interesting to give another extract from the Report [64]. An account of the results, after my third visit, will be found on page 106.

“ It may be advisable to correct some popular errors regarding the operation of clearing mosquitoes. No one has ever supposed it possible to exterminate mosquitoes from whole continents, or even from large rural areas—the operations must be confined principally to towns and their suburbs. No one imagines that it will be possible to exterminate *every* mosquito even from towns—we aim only at reducing their numbers as much as possible. No one supposes that it will be invariably possible to drain or otherwise treat every breeding place of mosquitoes in a town ; but even where every place cannot be dealt with, it will always be possible to deal with a very large number ; and it often happens that the smallest and most easily drained or emptied puddles or pots breed the greatest number of mosquitoes. Mosquitoes may possibly be carried into towns from a long distance by winds, though I

doubt whether there is much or any reliable evidence in favour of this view ; but, as a general rule, the vast majority of mosquitoes existing in a town are bred in the streets, yards, gardens, and houses of the town ; and if we get rid of these breeding places, we may calculate on at least greatly reducing the insects in the town. These are the simple principles upon which our efforts are based."

CHAPTER XII

FREETOWN, 1901-2

IMMEDIATELY after our return to England in September 1901, I wrote my Report [64] and then compiled my little book [68] which was intended to enable anyone in the tropics to do the work which we had been doing in Freetown. It compared the various methods of malaria-control and described some campaigns then being commenced. The whole edition of [68] was gradually sold out ; but I fear that it did not stimulate as many men to follow our example as I hoped it would have done.

In October, according to my promise (p. 89), the School sent Dr. Everett Dutton to British Gambia for mosquito-control work. The Governor, Sir George Denton, would not allow me to subscribe money out of my Tropical Sanitation Fund for this. In December 1901, Dutton, who was an able and enthusiastic investigator, discovered, with Mr. R. M. Forde, the Colonial Surgeon, the first-found trypanosome in human blood (*Trypanosoma gambiense*), which was later proved to be the cause of sleeping sickness. He returned home about the end of January 1902.

We experienced greater difficulty in finding anyone for the Gold Coast (p. 97) ; and when we did he

appeared to be under a misapprehension regarding the work he was required to do. We were obliged to withdraw him, and to send Logan Taylor in his place. Mr. F. Swanzy, who had added £500 to my Fund for that colony, was, like the Governor, much annoyed ; and so were we. There were few expert "malariologists" in those days.

I had been appointed President of the Tropical Diseases' Section of the British Medical Association's meeting at Cheltenham (30th July to 2nd August 1901), but could not attend as I was absent in West Africa at the time. I sent them a Note, however, on the *Habits of Europeans in India and Africa in Relation to Malaria*, in which I pointed out that the "habitual use of punkahs, mosquito-nets, well-built houses, and comparatively good food" by Europeans in Indian cities puts them at an advantage compared with their fellows in Indian plantations and in African coast towns. They are generally segregated from natives and possess gymkhanas, clubs, dairies, vegetable-farms, mutton-clubs, ice and soda-water in the large Indian stations, while many of these were frequently wanting in African settlements ; and I argued that "the slow progress of the African colonies is chiefly due to the stupid indifference to these details" and not so much, probably, to the greater "disease potential" there. So I still think ; but truth is an unpopular goddess with *Homo insipiens*, and my paper was only "*taken as read.*" There were, however, several valuable papers given at the meeting.

On the 21st October I gave similar unwelcome advice to the African Trade Section of the Liverpool

Chamber of Commerce [65]. But many of the business men present told me that they would act on my advice regarding their employees.

Alfred Jones was knighted (K.C.M.G.) about the middle of November 1901.

About that time several writers were attacking me in the London papers : some seemed to think that I was to blame for the fact that mosquitoes carry malaria, and others that I was a charlatan, who was amassing a large fortune by my pretences. The doctors were jealous, the medical press was cold, and the Colonial Office was supercilious.

On the 18th September 1901 I attended the meeting of the British Association at Glasgow and read a paper on malaria-prevention and on our work at Sierra Leone. Lord Lister, who was present, moved the vote of thanks to me. As I have said, he had closely followed my work and writings ; and I look upon his remarks on that occasion as the red-ribbon of my life.

“ He wished to bear his testimony to the qualities which had enabled Major Ross to bring about this great discovery, because the discovery of the development of the parasite in the mosquito was due solely and simply to Major Ross, who had shown admirable scientific acumen, zeal, and perseverance. At the same time he had—very differently indeed from some Italian investigators—shown absolute candour, perfect openness of mind, and a readiness to recognize the work of others.” (Times, Thursday, 19th September 1901.)

I received several letters in 1901 on the priority question besides those from Koch and Laveran quoted in Chapter IV. Dr. Mannaberg, of Vienna, one of the

best writers on malaria, wrote to me on the 6th June : " It seems that the Italians * are attempting once more what they already did twelve years ago with Laveran." On the same date Dr. E. Almquist, of Stockholm, who had asked me for papers on the subject, wrote : " In the question of priority there cannot be any doubt. The matter of fact is so easy to demonstrate, and Koch and the greatest authorities are of your meaning." On the 17th June, Dr. Galli-Valerio, of Lausanne, a capable worker, wrote : " For me, it was not necessary to read it [61] to have the conviction that the merit of the great discovery is to Major Ross. And I am sure that many other Italians think the same."

On the 7th February 1928, with the consent of the donors, I handed over the whole residue of my Tropical Sanitation Fund, with accounts and vouchers complete, to the School for future administration under the conditions for which the money was originally given.

I could not afford to pay my own expenses again for my third visit to Africa, which I wished to make in order to inspect Logan Taylor's work. Mr. Coats gave me permission to draw up to £50 for this purpose. We had heard such fine accounts of the work at Freetown from the Governor and from Dr. Logan Taylor that I decided to take my wife with me this time just to show that I, at least, did not consider Freetown to be any longer " the white man's grave."

We left for Freetown on the 22nd February, but we feared (or rather hoped) that we should die on the way out. We were put into a nice cabin on board our ship, in which the stewardess, who had a bad cold,

* He meant *some* Italians.

had slept in port. A day or two later my wife and then myself went down with the most appalling malady of the same class, which prostrated us until we neared Freetown. On landing there on the 7th March the heat and dust produced a relapse worse than the original attack. Sir Charles and Lady King-Harman were kindness itself; but the upper story of Government House, in which we slept, was dreadfully hot; and we were more or less ill during the whole of the nine days we spent in Freetown, and scarcely revived until we approached Liverpool again at the end of March. These are the facts, but our friends, the sceptics, in Sierra Leone immediately put it about that we had been attacked with malaria there, and this lie actually appeared in the papers. It was afterwards traced to its source—a doctor in Freetown who knew the truth of the case but had always opposed the anti-mosquito campaign. I was only just able to inspect Logan Taylor's admirable work, but was obliged to abandon the visit to Bathurst which I had hoped to make on the return journey.

I reported the results obtained in a letter to Jones, which was published in the Liverpool Courier on the 16th April 1902. Speaking of Taylor's work I said:

“Employing about 70 men (of whom twelve have been lent by the Governor, the rest being paid by the School), he has drained nearly the whole of the most pestilential parts of the town. These parts were the flattest areas of the valley in which Freetown lies, and were full of hollows, pits and ill-made drains which in the rainy season contained numerous pools of stagnant water, breeding swarms of the malaria-bearing mosquitoes, *Anopheles*. So shocking was the condition of affairs that many of the streets were prac-

tically marshes in the rains, the houses being situated in the midst of seething puddles full of mosquito-larvae, frogs and tadpoles. . . . In fact it was very largely these drains which were responsible for the bad sanitary reputation of the 'white man's grave,' many of them being huge square trenches without any adequate fall and containing innumerable 'pockets' holding filthy water for months.

"How anyone expected that Freetown would be otherwise than unhealthy when in such a condition it is difficult to imagine. . . . Dr. Taylor informs me that these men (the *Culex* gang) have now removed 2,257 cartloads of such rubbish and have visited 16,295 houses—the latter figure implying, of course, that all the houses in Freetown have been revisited periodically. . . .

"It will now be asked, what effect have these measures had, after eight months' trial, on the number of mosquitoes in Freetown?

"In my opinion we have absolutely demonstrated the possibility of getting rid of mosquitoes at a small cost in Freetown, and therefore probably in any town. . . . By getting rid of mosquitoes I do not mean that not a single mosquito is to remain. Such is, of course, an absurdity, because, even after the most careful search, a few breeding places will always remain undetected, especially in the houses of people who persist in keeping stagnant water in pots and tubs, and who are too lazy or stupid to look after their own comfort. I mean that the number of insects can be reduced so largely in towns that they will cease to be a cause of disease and discomfort to the bulk of the population.

"We will not attempt to give any numerical estimate of the decrease of mosquitoes in Freetown because there is probably no sound way of computing the actual number of mosquitoes anywhere; but the general concensus of opinion is that they have been very greatly reduced there. A number of people, both Europeans and natives, informed

me that they had seen no mosquitoes for months. In our rooms at Government House, where I had been frequently bitten on my first visit, we neither saw nor heard a single mosquito during nine days, though the house is surrounded with trees and though our windows were kept open all night. I never remember to have had a similar experience during sixteen years' life in the tropics. Natives residing in the recently-drained areas told me that there were no longer mosquitoes in their houses. I requested Mr. Shaw, Dr. Taylor's assistant, to procure for me as many *Anopheles* as possible from the houses of the natives for study, but after several days' search he could bring me only one! though he and his men had searched a number of houses built close to the streams, in which houses Drs. Christophers and Stephens had found swarms of *Anopheles* in 1900. Of course, as already mentioned, mosquitoes must still be present here and there where their breeding places have not been detected (and in fact a few such are sometimes seen in a corner of the verandah of Government House itself); but there is undoubtedly a vast reduction of them in the town as a whole, and this is the main thing. In the rains, however, we must expect a temporary increase. . . .

“As a matter of fact these insects are strictly local in their ordinary habits, and it is obvious that where we reduce their breeding places to any great extent, we must also reduce their numbers to a similar degree. I fancy that the breeding places in Freetown have been reduced by at least ninety per cent, and it would be surprising if the insects remained as numerous as before.

“As an instance of the imaginary difficulties cited against us, I may mention the case of the mountain streams, in which numbers of *Anopheles* larvae were found in 1900. It is, of course, impossible to drain these streams, and hence it was immediately assumed that this fact would be an insuperable bar to operations against mosquitoes. Dr.

Taylor, however, tells me that six men suffice to keep these streams free of larvae within the town area by periodically brushing out or oiling the pools. Out of the town area the streams are simply left alone, such mosquitoes as breed in them being too few to trouble the town even if they could easily reach it through the forest and bush. Contrary to the popular idea, mosquito larvae are not usually found in such swamps as exist near Freetown.

“The cost of all these operations has been very small. Labour costs only about one pound a head per month. To this must be added the salary of Dr. Taylor as superintendent; but under normal circumstances, the health officer and engineer should superintend the work. After the drainage system is complete, the number of men employed in the campaign against mosquitoes can of course be reduced. One of the principal causes of expense should be the quantity of cement required for the works; but owing to your gifts of this material we have spent little on it. The expenditure of oil, in spite of its extensive use by Dr. Taylor, has proved to be very small; and only one and a half barrels out of the tons of petroleum and creosote generously provided by Mr. John Holt having been used as yet. In fact crude petroleum is a most economical culicicide, an ounce or two sufficing for many puddles. Indeed I am sure that by far the most economical way of dealing with malaria in towns and the best, is by clearing away the breeding places of the larvae of the insects which carry the disease. . . .

“Now as regards the effect of the anti-mosquito measures on the health of Freetown: I have previously said that we must not expect a sudden disappearance of a relapsing disease like malaria similar to the sudden disappearance of yellow fever which followed anti-mosquito measures in Havana. Nevertheless it is generally admitted that the health of Freetown has been remarkably good. A number

of people assured me that they had not suffered from fever for a long time.

"Dr. Taylor, though he has been constantly engaged in 'turning the soil'—a thing which people imagine is sure to produce fever—has not had a single attack since his arrival." . . .

I concluded this letter by saying that the Europeans in Freetown seem to be much more cheerful. I gave some health statistics but, of course, no decision could be based upon such figures as had been kept in Freetown by the medical authorities there.

When this letter reached Sierra Leone it was received with the usual protest by a writer in the Weekly News of that Colony, who, speaking as an "old resident," attributed the improvement to "general sanitation" and "flatly contradicted" the hypothesis that he was at all more cheerful now than before! To this the editor added the note: "We take the liberty of saying that we have been here quite as long as our Correspondent; having been born here; and we are in a position to state that the sanitary activities of Major Ross and Dr. Logan Taylor have had a marked and important effect upon the health and spirits of the Europeans—at least of all whom we have had the honour to meet."

We kept Logan Taylor at Freetown for the appointed year and did all we had designed to do. We had greatly reduced the mosquitoes, cleaned up the town and delivered our object-lesson: but whether the local authorities would continue the work depended, as we had warned them, entirely upon themselves. They showed no signs of doing so; and in a paper

which summarized his work and conclusions (British Medical Journal, 15th Sept. 1902) Taylor said clearly that even after one year's free scavenging of their town, the Freetown authorities do not seem in the least inclined to do it for themselves.

The excuse for their inaction which they always gave was that it was proposed to provide an elaborate engineer's drainage scheme for Freetown when money was available, which was not then. I had seen too much of such schemes in India to believe much in them for mosquito-control. As a matter of fact, the old drainage system which was in Freetown when we had arrived was itself responsible for most of the *Anopheles* in the town, which bred plentifully in the square stone-gutters. I was struck during one of my visits at observing that in a suburb called King Tom's Land, to which the old drainage-scheme had *not* been extended, there were many fewer mosquito-pools than in Freetown itself, because the rain and dirty water used to soak straight into the ground in the suburb and did not run into the filthy gutters which had been provided by "previous authorities" in the town.

I knew something about the matter because I had specially studied surface-drains and had written a report on them when I was in Bangalore in 1896.

Besides these facts it is a delusion to think that an engineering scheme of drainage is exactly the same as a scheme of *Anopheles*-control, because the insects often breed by millions in shallow pools or runnels of water which may be ignored by an engineering scheme.

A single shower in Freetown would suffice to fill it

with such pools and puddles and runnels suitable for *Anopheles*, but apt to be ignored by the engineers.

Doubtless a really good engineer's drainage scheme would have been very useful though probably not sufficient, but as it was entirely in the air at that time, we were right in doing our best under the conditions which we had actually found. What has been done since that date I do not know.

Mr. Coats's fund being now nearly expended we sent Logan Taylor on to the Gold Coast, where the Governor Sir Matthew Nathan wanted him (p. 103).

When the Fund was finally exhausted we recalled Taylor to England and closed the expedition (April 1903).

CHAPTER XIII

ISMAILIA, 1902

ALL the time since 1899, I had remained only a Lecturer on Tropical Medicine at Liverpool with a salary of £300 a year besides my pension of £292 a year. My age was over forty and I had four children to bring up. There was no evident desire to endow a professorship with a pension for me as Boyce had promised when I left India, though the School was now receiving subscriptions amounting to £2,500 a year, chiefly due to my work and ideas (see the *British Medical Journal* of the 6th May 1899). Both Lord Lister and Sir William MacGregor thought I was being exploited. I considered that I had now done enough against malaria in West Africa for nothing—I was a lecturer, and not a sanitary knight-errant. In December 1901 I therefore threatened to resign and to take consulting practice in London, unless my position in Liverpool could be improved. Just as I was leaving Liverpool for my third visit to Africa, however, I received a letter from Lord Lister offering me a laboratory for the study of animal parasitology at the Jenner Institute of Preventive Medicine in London (of which he was Chairman), with a salary of £500 a year and with an assistant. I had no time to

consider further details, but accepted the post just as our ship was casting loose. When I returned therefore I resigned my Liverpool post and went to London, but speedily found that the new post was not so satisfactory as I thought. It was not to be *ad vitam aut culpam* like a professorship, but could be determined at the will or whim of a committee. I could not write to Lord Lister on the matter as he had gone to South Africa on a visit. Meantime Boyce, Jones, Milne and especially Professor (now Sir Charles) Sherrington, recently President of the Royal Society, had been making strenuous efforts to found a Chair of Tropical Medicine in Liverpool, and made me to understand that I should probably be given the Chair if I resigned the Jenner Institute, which I did in June 1902. Lord Lister wrote me a charming letter when he heard of the incident. I have my own opinion as to why he took the action he did.

I received the Companionship of the Bath from King Edward VII on the 24th October 1902; and was awarded the Cameron Prize of £80 by the University of Edinburgh on the 26th July. Sir William MacGregor was present to receive an honorary degree. I spent my prize in buying a Powell and Leland microscope with Zeiss lenses.

Now came the ideal proof, both of the malaria-mosquito theory and of malaria-control by mosquito-control. When I was at the Jenner Institute at the end of April 1901, I received a letter from Prince Auguste d'Arenberg, President of the Compagnie du Canal Maritime de Suez, asking for my advice regard-

ing the great increase of malaria during recent years in Ismailia—the headquarter-town of the company, which had been founded by Ferdinand de Lesseps, and was situated about half-way along the Canal close to Lake Timsa. The representative of the company in London came to see me and I finally agreed to go there soon. After my return to Liverpool I decided to give up some of my vacation in order to be at Ismailia at the beginning of the malaria season in the latter part of September, and Sir William MacGregor, who was going to Italy, said he would join me. Accordingly I left England on the 12th September and was joined at Brindisi by Sir William. We reached Port Said on the 17th September and breakfasted at the Company's quarters, where we were almost torn in pieces by swarms of *Aedes* mosquitoes, which were evidently breeding somewhere near in spite of all I had written. The same day we proceeded to Ismailia, where we were housed in the palatial residence of the President himself (who was absent in Paris); but the *Aedes* bit us all day, and the *Culex* in the evening, with a few *Anopheles pharoensis* occasionally.

Built a mile or so from the great canal upon what was originally absolute desert, Ismailia had become in 1902 a well-built, scrupulously clean, little town of 7,000 inhabitants, of whom 978 were French employees and their families. The natives occupied a separate quarter, also well-kept, and the Europeans were housed as well as was to be expected from the great wealth of the Company and the capacity of its president (who resided here for many months every

year)—large mansions, beautiful gardens, perfect roads, under a rainless sky and not a rubbish-heap to be seen, or a smell to be smelt, the very pink of good conservancy. Yet even here we were almost lifted off our chairs by the mosquitoes, while the malaria which had commenced with 300 cases in 1877 (when the fresh water canal had been introduced) had increased to 2,284 cases in 1900! It was even proposed to abandon the town.

Two years previously the Company had commenced to drain marshes round the town—no good. Early in 1902 they commenced general quinine-prophylaxis—very little good. The same year, one of the junior medical officers, Dr. A. Pressat, was sent to study the wisdom of the Italians, who, of course, advised more quinine. This officer was now recalled to meet me, but these gentlemen had never quite understood my basic principle that where the mosquitoes are, there will the larvae be also. After discovering a few *Anopheles* larvae and removing a few pots of water, they observed that the insects remained as numerous as before, and therefore gave up the task and sent for me. Within two days Sir William and I, accompanied by a train of officials and workmen carrying creosote and petroleum (which I had brought with me), found numerous larvae of *A. pharoensis* in a water-cress bed in several small irrigation pools and in a little shallow marsh of fresh water oozing from the fresh-water canal. There were none in the canal itself, owing to the fish, and we showed the staff exactly how to deal with the *Anopheles*. Next came the question, whence did the *Aedes* and *Culex* come? Not

a larva was to be found above-ground. There were no rain-water tubs, gutters, or cisterns, because there was no rain. The officials said that they came from miles away. But I had remarked that there was an excellent water-carriage drainage system *without sewers* ; where then did the sewage go ? Into well-constructed pits called *puits perdus*, situated under each house and hermetically sealed, except for a long ventilation pipe opening above the roof. Up went the sealed flagstone over the manhole of the pit, down went our bucket and up it came again with a swarming, wriggling mass of larvae. This was where the mosquito-devils bred ; and after hatching they would fly up the whole length of the ventilation-shaft to torment the (rather dull) Lords of Creation, and then fly back again to lay their eggs. A tumblerful of oil for each pit once a week or so was enough. So much for Ismailia.

We also found *Anopheles* in a small fresh-water swamp near Suez, particularly in hoof-holes in the mud. We went to Cairo (which is not malarious) and failed to find *Anopheles* in a marsh there, though it seemed in every way suitable.

I dissected many *A. pharoensis* at Ismailia. I failed to find any *Plasmodia* in them but showed Dr. Pressat how to perform the little operation.

Sir William and I gave the officials a parting dinner, at which Sir William, who was no gourmet, had ordered the red wine to be iced ! At this banquet I promised them complete relief from malaria. They said the wine ought to have made me pessimistic instead of optimistic, but I was right ; even in the last quarter

of 1902 the malaria sensibly diminished. By July 1903 the number of malaria cases had been considerably reduced, while the *Culex* mosquitoes had been suppressed in a manner nearly absolute. The following table gives the actual statistics for 20 years :—

Years	1877	1878	1879	1880	1881	1882	1883	1884	1885	1886
Cases	300	400	500	400	450	480	550	900	2000	2300
Years	1887	1888	1889	1890	1891	1892	1893	1894	1895	1896
Cases	1800	1400	1450	1900	2590	2050	1750	1100	1350	1150
Years	1897	1898	1899	1900	1901	1902	1903	1904	1905	1906
Cases	2089	1545	1545	2284	1990	1551	214	90	37	0

After this there were no cases contracted in Ismailia for a number of years. I visited the place again in 1909. Further details will be found in [91] by H. C. Ross together with an account of a similar success in Port Said due to E. H. Ross in 1906.

Dr. Pressat, in a pamphlet (Masson et Cie, Paris, 1905) said that the essential work in the town was done only by four men under his direction and was maintained by them. Thus was saved the City of Ferdinand de Lesseps. The fee I asked for visiting Ismailia was no less than £100, but the Canal Company added my out-of-pocket expenses to it, and from that time has subscribed £40 a year to the Liverpool School of Tropical Medicine, I believe. Thus do the slaves of science toil for others' benefit.

While we were in Ismailia Sir William and I anxiously discussed the future of malaria-control throughout the Empire. Obviously no general action would be taken unless the central authorities in England insisted upon it. He therefore suggested that on his return to England he would ask the Colonial Office to allow

him to give up the Governorship of Lagos and appoint him Malaria Commissioner for the Empire, with a Sanitary Engineer and myself as his Assistants, and he hoped that the India Office and the War Office could be got to join in the scheme.

After parting from me he studied malaria-control in various places in Italy and then went to Amsterdam (p. 89).

The Colonial Office would not allow his scheme and he hinted that there was much jealousy of me in London and a "hidden hand" against me somewhere. For the world I was sorry at his failure—for myself, relieved. He returned to Lagos next year, where his health soon broke down.

In 1902 I delivered many public lectures on malaria and mosquitoes organized by Mr. Gerald Christy of the Lecture Agency in London.

On the 2nd December I was made full Professor in University College, Liverpool—which was now being converted into a University (Charter, dated 15th July 1903).

But Fortune, not content with giving me so many nice things (with a knock or two), now suddenly conferred upon me the greatest and most unexpected of honours—the Nobel Medical Prize for 1902. My wife and I went to Stockholm to receive it on the 10th December (Nobel Day). The Nobel Album, Nobel Gold Medal and Prize were handed to me and other recipients by the King of Sweden himself, and we were enthusiastically treated wherever we went, particularly by Professor Count Mörner and his wife, and by Professor Ribbing of Lund, whom we called the

Senior Student of the Liverpool School, and his wife.

The account of all this business will be found in my Memoirs [101].

CHAPTER XIV

PANAMA, 1904

NOTORIOUSLY, men are so accustomed to false opinions and even to false statements that they do not accept any opinions or statements until confirmed by independent witnesses. Years elapsed before the general public accepted my scientific work ; many more years passed before it could be persuaded to believe that malaria could be reduced anywhere by mosquito-control. Both that measure and myself remained very unpopular among British officials everywhere : governors disliked the expense, and doctors the trouble. They combined not against mosquitoes, but against me ; and all my old adversaries, those whom I called the blue-mistics, the Grassians, and the vacuum-theorists, helped them. For years I was frequently and successfully demolished in the press. On no less than four occasions have I been obliged to seek legal advice against the bitter attacks made on me. For the similar cases of Colonel W. G. King and Mr. W. W. Haffkine, see my *Memoirs*, pages 243 and 487.

All sanitary advances are unpopular. Sanitation is world's business and therefore nobody's business. Reformers are frequently treated exactly as shown in Ibsen's comedy, *An Enemy of the People*. Even the

most brilliant results of sanitary science are of a negative character ; not something tangible like a new post office, school, or hospital, but a mere absence of what few people ever see—illness and death. Gorgas told me in 1904 that after he had cleared Havana of mosquitoes an American official said to him, “ I don’t know why you make such a talk about mosquitoes and malaria : I have been here a week and have seen very little of either ! ”

I stated on p. 112 that after we had delivered our object-lesson in Freetown by cleaning up the town, chiefly at our own expense, for a year, we had closed our work there and sent away Logan Taylor in 1902 to the Gold Coast much dissatisfied with the inactivity of the Freetown authorities (*British Medical Journal*, 28th September 1902), and that we finally closed the expedition and withdrew him from Africa in April 1903. A few weeks later, a letter reached the Malaria Committee of the Royal Society from the Freetown Authorities, who praised themselves, declared we had done very little, and now demanded a commission of enquiry on our work ; and the Colonial Office asked the advice of the said Committee as to whether such a commission should be appointed. The Malaria Committee would not listen to what I had to say but decided in favour of the enquiry.

So far as I remember, the Committee consisted entirely or mostly of pathologists and entomologists without any experienced public-health officers ; and I had even suggested my resignation of it to Lord Lister (letter of the 24th January 1903). He was not present on the occasion referred to.

Of course, as everyone acquainted with practical sanitation would know, the Sierra Leone proposal was not only impossible but absurd and dangerous. No commission could possibly gauge the value of our work a year after it had been discontinued—a year after the local authorities had allowed the town to revert to its original condition. The oil which we had put on the puddles would have evaporated, the channels we had cut or cleared, the drains we had improved, and the gutters, would have become choked again with weeds and stones, the backyards would have been filled up again with mosquito-breeding rubbish. The doctors there had kept no reliable statistics and had refused to help us by taking “spleen-rates,” so that the effect of our work on the public health could not be estimated. In fact the proposal was mere white-wash or worse. We should be condemned if we were to object to it and, if not, the commission would enjoy the hospitality of Freetown, learn all about our misdoings in our absence, and be in the position of a judge and jury who dine with the plaintiff before the trial.

The great danger was that they would end by condemning mosquito-control altogether. That would be the death not only of my efforts, but of those of Sir William MacGregor. So I went home to Liverpool and wrote two strong letters on the 19th June and the 9th July 1903 direct to Mr. Chamberlain, who quashed the proposed commission entirely. I had been proposing to renew my Tropical Sanitation Fund and my visits to West Africa ; but this experience ended my projects regarding both ; and such was all the thanks that Logan Taylor and I ever received from that (very)

dark continent. But some of the slanders evidently took effect, for Mr. Coats who was so generous at first, would give us no more help.

On the 29th October 1903, however, Mr. and Mrs. Chamberlain paid a surprise visit to Liverpool, and Lord Derby (the sixteenth earl) asked Jones, Boyce and myself to meet them privately at lunch at the Adelphi Hotel. The great Minister with his cigar and eyeglass was a plagiarism of his caricatures, as I was still considered by my countrymen to be a caricature of the ineffable Grassi ; and he acidly disputed some of Jones' schemes for West Africa until the latter said laughingly : " Well, Mr. Chamberlain, everyone knows that you snub us all but that Mrs. Chamberlain smooths us down again ! " He was complimentary about our sanitary work in West Africa (evidently to make amends for Freetown), but when I pressed him regarding sanitary commissioners for West Africa (p. 83) he said that he was not going to set spies over his African officials (sic) ! Of course, inspectors are necessary for all good management, for navies, armies, schools, down to municipal sanitation, shops and working gangs, all of whom might be described as spies.

When Jones and Boyce hinted that the only way to get anti-malaria work properly done in Africa was to send me out there in an executive capacity, Chamberlain changed the subject ; and I concluded that further efforts were hopeless. It did not matter to me personally how many people died there, and I had already done as much as a private individual could do in this line and as—well, as my countrymen

deserved. Sir William MacGregor entirely agreed with me when I told him later of the interview—which had no results whatever. Thus it was that the glory ultimately passed to Gorgas and the Americans.

I undertook many more lectures that Autumn (combined with nutritious banquets): Anderson's College, Glasgow, the Royal Colonial Institute, the Royal Engineers, the Clinical Society of Newcastle, the Sanitary Institute at Bradford, a Medical Congress in Brussels, the British Medical Association. We tried without avail to get up a joint petition to Government. Sir William MacGregor renewed his efforts to form an Imperial Sanitary Commission also without result. He said that all this availed nothing against the hidden hand. In 1904, he was transferred from Lagos to Newfoundland on account of his health.

Meanwhile most of the doctors, especially in India, were preaching the impossibility of reducing mosquitoes anywhere, because, they said, if we cleared them from one spot, they would rush in from outside like a gas into a vacuum. I therefore made a mathematical study of the question, which resolved itself into the following problem: If a million mosquitoes be liberated from a box in one spot and be allowed to wander freely in all directions under equal conditions until they die, then how far from the box will their dead bodies be found? It was a problem in the Law of Chances, and after much labour (I could get no mathematician to assist me) I found a tentative solution: that their numbers would be greatest near the box and would diminish as a function of the distance away from it. I called this the Law of Random-

Migration ; and my paper, which showed by means of simple proof and diagrams the absurdities of the vacuum-theorists, was read at St. Louis in 1904 and was published next year [80]. But as the Law had several wide applications even to the Theory of Evolution, I asked Professor Karl Pearson early in 1904 * to give me a better mathematical solution. He gave it in a pamphlet in 1906 and confirmed my results by means of much more elaborate analysis [80]. Mr. W. Stott sent me another solution still later. I have not studied either solution very carefully (see p. 154).

But all this was above the heads of our opponents and they soon invented another war-cry. Nothing was to be attempted against mosquitoes until the native populations were sufficiently educated—which will be, never ! One might as well postpone street-sweeping, sanitary-cleansing, and the making of sewers indefinitely until the millennium. Even Manson was taken in (see for instance the *British Medical Journal*, the 10th August 1907, page 388). The cry is still repeated by people who have no experience of practical hygiene.

But consolation for India and West Africa began to arrive slowly in 1903-4. Malcolm Watson cleared Klang and Port Swettenham [91, 100] ; Hongkong was much improved [91] ; Andrew Balfour wrote to me the 8th February 1904 that he had commenced mosquito reduction at Khartoum [91] ; and Gorgas had scored a great success at Havana [91].

In 1904 I arranged to go to the Great International Congress of Arts and Sciences to be held at the World's

* Not in 1905 as Professor Karl Pearson subsequently stated.

Fair in St. Louis, U.S.A. ; and after I informed Gorgas of this fact an invitation reached me, either from him or Surgeon-General Walter Weyman, Head of the American Public Health and Marine Hospital Service (original letter lost), to go on to Panama afterwards. I did both but dismayed my medical audience at the Congress by reading my mathematical paper [80], which none of them understood, instead of giving them the usual summary of past work, though I was to have opened the whole discussion on Pathology—I had been led to believe that I might choose my own subject. But I met numbers of distinguished American doctors at the Congress, Osler, Councilman, and Thayer, among others. We drank beer in Swiss chalets and tea in Chinese pagodas ; but after a few days I escaped still alive with Osler in order to spend a few days with him at Baltimore. One of his young men took me to see Washington. I went with Dr. Musser to Philadelphia ; and embarked on the Steamship *Advance* for Panama on the 27th September.

Unfortunately Gorgas was then on leave and could not voyage out with me ; but he and Mr. Henry Clay Weeks, Secretary of the American Mosquito Extermination Society, came on board to see me off (see frontispiece).

Gorgas was a spare resolute man of the best type, and we discussed malaria-control for some hours. He was astonished to hear of the “ vacuum theorists ” and of the opposition in India and Africa, but said that Havana would defeat it, as it did. We reached Colon on the 4th October and Panama the same day. It was pitiful to see the parks of rotting cranes and

locomotives left by the French in consequence of the yellow fever and malaria, the latter being bred from innumerable stagnant pools and runnels between many small crumbling hills, with an immense rainfall. I saw some of the most distinguished "yellow fever men," H. R. Carter, who discovered "extrinsic incubation," J. W. Ross, T. C. Lyster, L. Balch ; and Mr. J. A. Le Prince very kindly showed me what was being done. I dined with General Davis, who commanded ; saw the Canal being dug ; lectured to a perspiring congregation on the 10th October and left for home on the 12th. There was little advice to give because everyone knew his work already. We came into the tail of a cyclone but arrived safely at New York on the 21st October, and then I transferred myself to the S.S. Arabic for home. Dr. Doty (see p. 89) gave us a triumphant send off and circled round us in his port steamer with flags and trumpeting like an elephant—till the passengers thought there must be a distinguished criminal, or duke, or even politician on board. We were home on the 29th October.

CHAPTER XV

GREECE, 1906

ON the 12th January 1905 Princess Christian and Mr. and Mrs. Joseph Chamberlain visited Liverpool ; and in the afternoon of that day I lectured at St. George's Hall on the Progress of Tropical Medicine, before them and Lord and Lady Derby, and a large audience ; and my wife and I dined at Knowsley.

My next excursion was to Greece in 1906. The Lake Kopaïs Company of London owned 60,000 acres in that famous haunt of eels and wished me to advise them regarding the malaria there. My wife and I had free tickets and out-of-pocket expenses, to which the Company kindly added £50.

We left Liverpool on the 18th May 1906, and reached Athens on the 25th May, and first visited the Acropolis by the setting sun, when the perfect mountains were of an olive green and the sea deep indigo blue ; and when the stars came out the sky was not blue but imperial purple. The President of the Greek Anti-Malaria League was my friend, Dr. S. Savas, Professor of Hygiene at Athens and Physician to the King. Dr. J. P. Kardamatis, the Secretary, accompanied us from Athens to the Kopaïc Plain on the 28th May with my friend Mr. B. Steele, the Agent of the Company

in Greece. We stayed in the Company's house at the foot of Helikon in a beautiful grove, where I committed my greatest sin of throwing a stone at a nightingale which annoyed me all night, and would not let me sleep.

That was mere pleasure ; my work was more serious. I had to inspect the locality for the breeding-places of *Anopheles* and also to measure the malaria at Moulki—the neighbouring village—by taking “ spleen-rates ” of the children.

The process of examining children's spleens in villages is always amusing. When you first arrive they run away shrieking, the dogs bark, the fowls cackle, and irate mothers stand at their doors. Then one of your attendants catches one of the children and brings it forcibly to you, and you impress a penny into its dirty little palm, let it go, and smoke a cigarette. Presently all the children stand round you in a ring with finger in mouth. After yawning you pat one on its head, insert your fingers under its left rib, where the spleen is, give it a penny, and let it go. Presently you know the proportion of children with enlarged spleens in the whole village—a useful measure of the amount of malaria in a place, which I have always used. By the same artifices we can usually persuade the infants of *Homo insipiens*, even to allow us to take a droplet of blood from their dirty little fingers—another useful test. Soon one is beloved by the whole village, priest, headman, innkeepers, mothers, children, dogs, fowls, and fleas. On the 4th June we returned to Athens and left for home via Constantinople by the Oriental Express and were home on the 15th June.

We also took malaria-rates at Thebes, where there is very little malaria, and at Orkhomenos, where there is much. The average malaria-rate was 57 per cent., which is of course not so high as a common Indian malaria-rate, and we also found *Plasmodia* in the blood of many of the children [83]. We found few *Anopheles* breeding-pools at that season.

During this visit I had formed a theory which was of considerable interest to scholars. It is certain that so recently as 1866 malaria entered Mauritius, the idyllic island of Paul and Virginia, and completely altered life there, driving the planters from their villas round the coast into the central tableland. To my medical apprehension, Greece in the time of the Persian Wars could not possibly have been in the present condition; while now malaria haunts almost every valley and the course of almost every stream, except in a few areas like the Attic Plain, leaving only the barren hills, where there is little water, safely habitable. How could we imagine, for example, that wealthy Orkhomenos could have been as malarious in those days as it was now—when moreover there was no quinine? I argued therefore that the disease must have entered Greece about 500 B.C. by the introduction of *A. maculipennis*, or perhaps of infected soldiers or slaves from Asia; must have then crept slowly up the valleys and destroyed their rural prosperity as it did in Mauritius; and so may have played a considerable part in the subsequent decadence of Greece. On my return home I proposed to have the subject investigated historically. Fortune favoured us, William Osler was now the Regius Professor of Medicine at

Oxford. He invited me to lecture before the Oxford Medical Society, which I did on the 9th November 1906 [83]. We also raised £500 very quickly to help the Greek League; and what was even better, found a most able historical investigator—Mr. W. H. S. Jones, of St. Catharine's, Cambridge. Assisted by Mr. A. E. Shipley and others, he examined much ancient literature, and his book (MacMillan and Bowes), with an introduction by myself, appeared in 1907, with another book in 1909 [89]. Both books confirmed my conjectures. The Greek League did some good work with quinine distribution, and cleared malaria from the banks of the famous Ilissos at Athens by "training" its banks. I visited Greece again in 1913 and 1917 (pp. 142, 145).

In August 1906 Sir Alfred Jones took Boyce and J. L. Todd and me to Brussels to pay our respects to King Leopold II, a large subscriber to our School. The King and his suite received us on the 31st. One of the conversations was so amusing that I recorded it immediately afterwards. We were talking about the Canaries. Jones, who was short, energetic and direct, exclaimed, "We should work up those islands. Your Majesty should go with me there." The King, who was immensely tall, thin and regal (he spoke English perfectly), looked down at Jones, laughing, and said, "What! *I* go with *you*, Sir Alfred Jones." The latter was not a bit abashed, and when the conversation veered to sleeping sickness, cried, "*We* have done a great deal in this line, and *your Majesty* should follow us." This really nettled the Monarch, who exclaimed haughtily, "It is for me to lead and you to follow, Sir Alfred Jones." Jones never turned a hair, and

presently we went into the most wonderful lunch I have ever eaten, with sixteen courses. But I still remained hungry ; for if I laid down my knife and fork for a moment to speak to the King, a footman behind my chair instantly snatched the dish away ! The King struck me as being an extremely able Sovereign, who recognized that his people's welfare was bound up with his own.

A few weeks later my wife and I attended the Quatercentenary Celebration of Aberdeen University. I had a terrifying experience and was nearly made a Doctor of Divinity by mistake, on the 26th September.

CHAPTER XVI

MAURITIUS, 1907-8

NOW at last in 1907, ten years after I had found malaria-parasites in mosquitoes, I was asked by a British Colony, Mauritius, to advise it regarding its malaria; but even now I was invited only to advise, not to manage the work, and I heard that the invitation originally emanated from the French planters and officials of the island. My expenses were guaranteed, and I was to receive £1,000 for three months' stay in the island. Sir Alfred Keogh, Director General of the Medical Services, appointed Major C. E. P. Fowler, R.A.M.C., to assist me, especially regarding the troops in Mauritius. I left England on the 23rd October, travelling via Marseilles, and reached the Seychelles in November—lovely mountainous wooded islands, peopled by 24,000 negroes, with a death-rate of only 16 per thousand. There was no malaria there owing to the hilly nature of the ground. We reached Mauritius on the 20th November.

This island, of 705 square miles, is shaped like an inverted saucer, with a high plateau reaching 2,711 feet in the middle, surrounded by a rim of jagged hills. Its rich cane-fields supported a considerable French population and 260,000 Indians. The Governor and

some of the officials are British ; the planters owned many beautiful villas out of which they were being driven by malaria ; the whole place was humming with industry ; numerous trains carried business-men daily from the capital, Port Louis, on the coast, to cooler towns on the central plateau. Troops were stationed at Vacoas ; but in 1866 this paradise was overshadowed by the entry of malaria, hitherto unknown there, and the disease is said to have killed a quarter of the population of Port Louis in 1867, and then spread slowly round the coast and elsewhere. Prosperity declined ; and in 1906 the death-rate reached 40 per thousand.

Fowler and I took a pleasant little house at Vacoas, which was infested with *Aedes*, breeding in the wild pineapple flowers in the garden and elsewhere. We had great difficulty even in reducing these insects, and spent a strenuous three months visiting many places in the company of Dr. Lorans, Director of the Medical Department (who died a little later), and of M. D'Emmerez de Charmoy. The Governor, Sir Cavendish Boyle, gave us 6,000 rupees, with which we employed ten "moustiquiers" and 30 labourers. Further details are given in my Report [87]. I also lectured to the Medical Society of Mauritius ; and again before the Governor at Curepipe ; and before the Mayor and Councillors of Port Louis.

A battalion of British troops had arrived at Vacoas just before we did. There was a severe outbreak of malaria among them with 5 deaths and many invalidings costing thousands of pounds ; and yet the authorities go on allowing malaria to continue amongst their

employees. At last we left for home again, and reached London on the 28th March 1908. Of course we had to spend most of our time in Mauritius in taking spleen-rates ; but found *A. costalis* there and proved it again to carry malaria. Since then my friend Mr. Malcolm Macgregor has also found *A. funestus*, the other common Sierra Leone *Anopheles* in the island. We showed that *A. mauritianus* is negative. The general spleen-rate of the island worked out at 34·1 *per cent.* My final scheme for malaria-control was estimated to cost 135,000 rupees annually, or about £9,000 at 0·36 rupees per head of population, and 1·2 *per cent.* of revenue.

I do not know what the Colony has really done as regards acting upon my scheme of prevention ; but it also asked Macgregor to advise. He spent a longer time in the island than we did, and I trust that our advice will be followed ultimately.

Immediately after my return home I set about writing my Report [87] ; but as there were still many sceptics regarding the mosquito-theory of malaria I thought it advisable to put into this Report a rather full statement regarding the whole theory, including some elementary mathematical considerations (see Chapter XX). On completing this task, I commenced my book [91] ; but it was not completed and published till two years later.

CHAPTER XVII

INDIA, 1909

EXCEPT for isolated efforts by Colonel W. G. King and some others, little actual malaria-control work had been done in India since I left it in 1899. In 1902 some work of this kind was started at Mian-Mir, the military cantonment attached to Lahore—an absolutely flat area with an impervious subsoil which is covered with pools during the rains. It was probably the worst type of country for a test-case. The enterprise was inadequate and was adversely criticized by me and by others [77, 90]. Nothing further seems to have happened except that the vacuum-theorists and devotees of the great god, Non Possumus, continued to clamour against me. Therefore, happening to get an introduction to the Secretary of State for India (I will not say when), I spent an hour alone with him in his office pleading my cause on behalf of the million people and more who are said to die in India annually, and of the millions still more, mostly children, who suffer from it every year. He sat before me like an ox with divergent eyes answering and asking nothing—and ended by doing as little. He was the personification of the British nation in the presence of a new idea ; and as I left I could almost

fancy seeing the prophetic handwriting on the walls over his head, "Mene, Mene, tekel upharsin."

In 1908 I was invited to attend a great medical congress to be held in Bombay in February 1909—my return fares to be provided; and, hoping that something might come of it, I made the long journey. Myself and several other delegates were most hospitably entertained by the Governor of Bombay, Sir George Sydenham Clarke (now Lord Sydenham), and I was delighted to find no mosquitoes in the house. But when I read my paper at the Congress on the 22nd February I was attacked by all the devotees mentioned in a united body. Subsequently I heard from several others at the Congress, who were indignant at this treatment, that the whole matter had been arranged beforehand and that I had been sent for in order to be publicly baited.

In October of the same year, an Imperial Malaria Conference was held at Simla, where many resolutions were passed, but, I think, nothing essential was done. A commission was, however, appointed to report on the alleged experiment which was still being conducted at Mian-Mir, with a view to finding whether mosquitoes can really be reduced. Their report appeared next year and was destructively criticized by me and Colonel King in the *Lancet* [90], and in *ibid.* of the 3rd December 1910. It appeared that since 1905 the princely sum of £66 a year was allowed for the work. This, be it remembered, for a difficult country of 8 square miles occupied by 5 regiments and a total population of 16,000 people. The sum amounted to about 1*d.* per head of population. At the same time,

I estimated, the disease was costing Government four hundred times that amount for the military in the station alone. The report of the Commission was not competent. Early in 1911 Lord Crewe, who was now Secretary of State for India, visited my laboratory in Liverpool and agreed to receive memoranda from King and myself on the whole subject of malaria-prevention in India; these, dated respectively 4th and 3rd April 1911, were duly despatched, and I understand will shortly be published.

In 1909, Sir William MacGregor had been transferred from the Governorship of Newfoundland to that of Queensland. On the 21st April 1911 he wrote to me indignantly that he had spoken very plainly to a certain office on my behalf and also about the African Yellow Fever speculation—in which, like me, he did not believe.

CHAPTER XVIII

SPAIN, CYPRUS, GREECE, 1913

SIR ALFRED LEWIS JONES, Chairman of the Liverpool School of Tropical Medicine, died suddenly after a short illness on the 13th December 1909.

My colleague, Sir Rubert Boyce, had a paralytic stroke in September 1906, but recovered and was knighted in November 1906 ; but had another stroke and died on the 16th June 1911. Tropical medicine is much indebted to the memories of both of them.

After returning from Mauritius in 1908 I spent most of my time writing my book *The Prevention of Malaria* [91]. It was issued with contributions by twenty men who had made special local studies, including Gorgas, Howard, Celli, Savas, E. Sargent, A. Balfour, O. Cruz, and M. Watson, in 1910. A second edition with more mathematical details appeared next year.

I received promotion in my Order on Thursday, 6th July 1911, shortly after Boyce's death. We received 296 letters of congratulation.

In January 1912 I joined a large party of distinguished men for the Parliamentary visit to Russia organized by Professor Sir Bernard Pares. On the 29th January we had an interview with the Tzar and

Tsaritza at Tzarkoe Selo. The scene lives in all our memories.

Mr. W. H. Lever (subsequently Lord Leverhulme) became the Chairman of our School after the death of Jones.

Now, in 1912, we had spent 13 pleasant years in Liverpool and had many friends ; but the place was not the same after the deaths of Jones and Boyce. Assisted by Drs. David and John Thomson, and Dr. G. E. C. Simpson, we carried out many "enumerative studies" on malaria and on trypanosomes, under my direction [93] ; and Sir Edwin Durning-Lawrence gave us £1,000 for experiments on the effect of extreme cold on such affections. I followed Sir Patrick Manson as President of the Society of Tropical Medicine in London in 1909, and was a Vice-President and Member of the Council of the Royal Society in 1911-13. At that time also I tried to establish a Bureau of Tropical Medicine, but did not succeed ; but the Colonial Office created one under the capable direction of Dr. A. C. Bagshawe. About the same time I was made a member of several Colonial Office Committees, which I was later forced to resign because that Office refused to pay for our attendance at some of its Committees, while another Committee, of the British Science Guild of which I was Chairman, had decided that all such attendance should be remunerated.

At the end of 1912, I resigned my Professorship at Liverpool in order to commence practice in London.

Before starting work, however, I went on my next malaria expedition. Leaving London on the 2nd March, I travelled by train to Madrid, and thence on

to the New Centenillo Silver and Lead Mines at Linares in the Sierra Morena, the owners of which in London had asked me to advise regarding cases of Mediterranean fever which had occurred in this place. Thence I went on through Cordova to Algeciras and Gibraltar, where I took steamer to Alexandria and thence to Famagusta in Cyprus (30th March 1913). I went there by request of the High Commissioner, Major Sir Hamilton Goold-Adams, to advise regarding malaria, as I had done in Mauritius—my fee was £200 plus expenses. The moving spirit was Dr. A. Cleveland, the Chief Medical Officer. Dr. Patrick, one of the medical officers, was appointed to help me, and I was given the assistance of Mr. Mehmed Aziz—a young Mahomedan who had been educated in the States and spoke perfect American. After I left he was made Chief Sanitary Inspector of the island, and I understand that he still holds that post. I have no space to describe this delightful visit—snow-capped Troödos, (?) Olympus, the barren Messaorian Plain, the sprouting wheat-fields, the blossoming almonds, the bonny, Turkish children humming their lessons in the schools, the medieval ruins of Famagusta, the eternal Kythrean spring leaping down from the heights of Pentadactylon, the lofty castle of St. Hilarion, the ruined temple and foaming shores of Paphos where Aphrodite grew from the wind-blown froth, and the far-off snowy summits of Asia, white against perfect skies. There I did again the old work, took spleen-rates and retold the old story in a public lecture; and on the 17th April left for Egypt and thence for Greece.

Athens once more and the mountained marsh of

Kopaïs. I had a long interview with M. Venezelos—a gentleman, not like a politician, who listened to all we said. Not much seemed to have been done at Moulki, but the children certainly showed a smaller spleen-rate, probably owing to natural variation. I was home again on the 3rd May 1913.

Mr. Murray had asked me to edit *Science Progress*, a quarterly which had first appeared in 1906. My first number was that of July 1913, and I gradually introduced many new features, especially regular contributions by experts, and on particular branches of science. The magazine survived the war, and Miss Edith Yates has been its Secretary since I was connected with it.

CHAPTER XIX

THE WAR AND AFTER

IN July 1908 I had entered the Territorial Force as Major in the Medical Service, and in November 1913 was promoted to Lieutenant-Colonel in the same. From the 21st December 1914, after the outbreak of the Great War, I was appointed with that rank Consulting Physician in Tropical Diseases to the Hospitals for Indian Troops in England ; but in July 1915 was sent to Alexandria, with Capt. D. Thomson as my staff officer, for similar work in the hospitals there. When, however, the terrible outbreak of dysentery in the Dardanelles, which was occurring at that time, and for which principally we were sent, had ceased, I was allowed to return to England (30th November 1915) after a short visit to Salonika.

But in 1916, severe malaria appearing among the troops there, our great Chief, General Sir Alfred Keogh, G.C.B., tried to get information from me regarding malaria in those parts. Of course, I was then merely a medical practitioner and had not troubled to keep myself *au fait* with the prevalence of malaria all over the world, and therefore could give him little help ; but as numbers of cases were being poured into England, he started special hospitals for them in all

the eight British Commands, and appointed Professor Stephens and me, and a gentleman from Edinburgh, whose name I have forgotten, Consultants in Malaria from the 15th February 1917, the special hospitals for malaria being distributed between us. Later, I was given a room at the Medical War Office close to Blackfriars Bridge, where I was able to keep in touch with all developments.

In November 1917 I visited Salonika again on behalf of the War Office, and returned on the 18th January 1918, was promoted full Colonel on the 5th February 1918, and remained at the War Office till I was demobilized on the 17th September 1919, being then appointed Honorary Consultant in Malaria to the Ministry of Pensions. After the War we were engaged in treating thousands of pensioners for relapses of malaria ; and Sir A. L. A. Webb established on my suggestion a number of special clinics for the purpose, ably managed by Drs. G. Basil Price, R. E. Drake-Brockman, W. Broughton-Alcock, E. Marshall, D. H. Jamieson, and others.

I saw nothing of the fighting (except on the London Front), but was torpedoed (Memoirs, p. 510) when proceeding to Salonika in 1917, from Taranto, whither Colonel J. C. Robertson and my Staff-Officer, Captain F. W. O'Connor, had been sent with me in order to deal with a bad outbreak of malaria there also—which was successfully suppressed [97].

Of course we were not able to do much anti-malaria work during the war—the bullets proved as troublesome, though often not so dangerous, as the mosquitoes—and there was no money to spare. But we were able

to make important investigations on the treatment of malaria at the special hospitals, where we had hundreds of cases of malaria to deal with, and we concluded that almost any doses of quinine over 10 grains a day were sufficient to moderate or to prevent *attacks* while the doses were being given, but that the *malarial infection* continued for months in spite of all forms of treatment which we tried. One of the officers working with me, Colonel S. P. James, I.M.S., even tried ten daily intravenous injections of quinine on some cases, but they all relapsed when the injections were discontinued [97, p. 332] and [98]. Dr. Meredith Harrison also tried massive doses of quinine up to 100 grs. a day, partly by the mouth and partly by intramuscular injection, with a view to extirpating the infection. I thought his results were a little better than most of our results, but many of his cases relapsed notwithstanding [97, p. 336, *Aldershott*].

During 1918, twenty-two battalions of British troops, mostly badly infected with malaria, were moved from the Salonika front to the French front. Lt.-Colonel J. Dalrymple, C.M.G., O.B.E., was deputed to try to improve their health before they were sent to the front. Dalrymple decided that the whole of these battalions to the extent of 75-85 *per cent.*, probably harboured malaria-parasites and he consequently put them into camps near Dieppe and ordered daily dosing with quinine for every man in the battalions, and the latter were frequently reviewed by him and sometimes by myself. The doses of quinine were about 10 grs. a day and were continued for three months, after which the divisions largely recovered

their health and were put into the firing line—a remarkable tribute to the efficacy of that treatment [97].

Similarly Professor Stephens and his colleagues at Liverpool made exhaustive trials on the treatment of malaria by dealing with special points, one after the other, and noting results on numbers of cases—a body of invaluable work.

My health was never the same after I went to Alexandria in 1915, but I am still alive in spite of the blue-mistics and vacuum-theorists and anti-anti-mosquitoists.

In May 1912 I had been asked by the Board of Education to report on the finances of the London School of Tropical Medicine, as the Board proposed to give it a grant—which it did on my recommendation. My enquiry showed that many of the workers were receiving very poor salaries, without pension; and in the *British Medical Journal*, 7 February 1914, I called general attention to the way in which these and other medical investigators were being exploited by the British Public and Government.*

In the meantime, in order to rouse some sense of justice in the matter, I had formed the rash scheme of following the precedent of Edward Jenner, the discoverer of vaccination, who, early in the nineteenth century, had petitioned Parliament for compensation for professional losses caused by his scientific work and

* I had previously argued the case that every country should give rewards for unremunerative discoveries (*Br. Med. Journ.*, 8 Dec. 1906).

had justly received £30,000 in consequence. I did the same for my much humbler work, but, I confess, with some satiric laughter and full expectation of failure ; and forwarded my petition on the 8th November 1913, after consulting lawyers, to Mr. Lloyd George, then Chancellor of the Exchequer. Of course, Cleon, who squandered half-a-million a year on politicians, refused it (*Science Progress*, vol. x, page 315, 1915). After the war, I submitted it again to the new Chancellor of the Exchequer, Mr. Austen Chamberlain, son of the statesman who had so skilfully established schools of tropical medicine at other people's expense, who also refused it ; possibly the Grassian Episode influenced this result. Meantime the British Science Guild and the British Medical Association, which supported me, formed a conjoint committee to report on the general question of State compensation for unremunerative medical discoveries (*Science Progress*, vol. xiv, page 635, 1920) ; and, led by our revered and eloquent chief, Sir Clifford Allbutt, we went on deputation to the Lord President of the Council, Mr. A. J. Balfour, on the 2nd March 1920, but received no definite reply (*Science Progress*, vol. xv, pages 113 and 285, 1920, and *British Medical Journal*, 6 March 1920). He did not seem to have troubled to read our Report (drafted by me) ; but, on the 20th July 1921, stated in reply to a question in Parliament that he was not in favour of the course we had suggested because " the difficulty of apportioning merit for even the greatest of discoveries is often overwhelming ; monetary rewards would lead to jealousy instead of co-operation among research workers." Of course the

same arguments might be urged against the giving of any honours or rewards whatsoever, from Victoria Crosses down to Peerages (Science Progress, vol. xvi, page 286, 1921) ; and if men of science are not to be paid, why should wealthy politicians receive large salaries and rewards for public work which they should be honoured to do for nothing ? After this failure we approached the Royal Commission on Awards to Inventors ; but it refused to consider medical discovery and invention at all, because (it argued) doctors had always been noble enough to do such public work for nothing ! We may be sure that the lawyers on the said Commission do not follow their example.

For the reasons exhaustively discussed in our Report these decisions were most unwise. They amount to this, that while the nation is to continue paying very large sums annually on subsidies for current "researches" which may, or, more probably, may not prove useful, it is to pay nothing at all for any discovery, however important, already achieved by private persons without expense to the State ; that is, the nation buys eggs, though probably addled, but not chickens when hatched ! But the case is worse than this. Notoriously, many well-to-do persons make a habit when sick of "sponging" on the altruism of medical practitioners ; so, it seems, the wealthy British Empire is to continue sponging for ever on the altruism of medical investigators—both cases of what is called "chousing the doctor." But the English are not a "bright crowd" and are easily misled by their politicians.

Meantime, however, my friends Sir William Simpson, M.D., C.M.G., and Sir Aldo Castellani, K.C.M.G., M.D., wished to start an Institute in which I could work and wrote a letter to *The Times* asking for subscriptions. This was very successful after some delay, and we finally bought a house near Putney Heath, which is now the Ross Institute and Hospital for Tropical Diseases. The Institute was incorporated on the 20th November 1925, and H.R.H. The Prince of Wales opened it on the 15th July 1926. Lord Leverhulme was our first President; but on his sad death on the 7th May 1925 he was succeeded by the Duchess of Portland. Sir Charles MacLeod, Bart., is our Chairman, and Mr. W. Shakspeare is our Vice-Chairman, and Lord Cavendish Bentinck is our Treasurer, while Sir William Simpson, Sir Aldo Castellani and myself are Directors, and Sir Malcolm Watson is on the Council.

After the opening I was invited by the Ceylon Tea Association to inspect progress as regards malaria-control in plantations in that Colony. I arrived at Colombo on the 4th January 1926, remained in Ceylon for more than 6 weeks, and returned to London on the 7th March. Dr. J. F. E. Bridger, head of the Public Health Department of the Colony, and Mr. H. F. Carter, the Government Malarialogist, had been doing good work there as I knew already, and they called upon me directly I arrived in Colombo (see Annual Reports, Medical Department, Ceylon, and [104]).

Before I left, I advised that the planters also should have their own malarialogist to help Mr. Carter, and

PLATE IV



MALCOLM WATSON

Face p. 151]

recommended them to take my brother, Dr. H. C. Ross, who had been working in Egypt [91]. But he was taken ill shortly after arrival and died on the homeward journey (Science Progress, July 1928), and was succeeded by Dr. Clemesha, who has continued excellent work there.

Next year I made a similar visit to Malaya in order to see the famous work done by Sir Malcolm Watson. He drove me in his car through a large part of the country towards the latter part of that year. Then I proceeded to Calcutta, from which Dr. C. Strickland, of the Calcutta School of Tropical Medicine, conducted me on a round of visits and lectures to Assam, with the kind approval of Colonel Megaw, head of the same School. I was present at the opening of the Gate of Commemoration at my old laboratory in Calcutta on the 7th January 1927 by Lord Lytton, the Governor of Bengal, and was back in London on the 24th February. My whole journey by rail, road, and river from London and back extended to about 20,000 miles [105].

Quite recently the Ross Institute has decided to form a Central Industrial Anti-Malarial Advisory Committee with Sir Malcolm Watson at the head. This is what I have always advocated, and its principal object is to advise industrial enterprise in the tropics how to put into practice efficient anti-malarial measures. Sir Malcolm Watson has now joined the Institute as Principal of the Malarial Prophylaxis and Control Department.

Also I have been recently carefully considering the question as to whether it is more economical to adopt

general mosquito-control or some form of special mosquito-control aimed against any special local disease, which may be prevalent. The latter form of mosquito-control generally demands considerable entomological skill, and therefore the presence of a well-trained entomologist, so that on the whole it may be actually more expensive than general mosquito-control, which can be carried out almost by anyone. My article on the subject is appearing immediately [106].

Some time ago I began to despair of malaria-control anywhere until I reflected that *Homo insipiens* requires at least a quarter of a century to understand any new idea ; and there is now considerable evidence that progress will be faster in the future. One of the most encouraging facts is that in India, the Indians themselves are beginning to adopt malaria-control at their own expense—a thing which a few years ago I would not have believed possible. But, of course, it will still be many years before the world achieves the ideal which I have always held before my eyes since 1898—to get rid of the parasite entirely.

The rarity of endemic malaria in the British Isles has probably been the reason why the people and the politicians and doctors who manage those affairs, and who treat us, have shown so little enterprise regarding the prevention of the disease in British tropical possessions. I doubt whether more than about 1 *per cent.* of these areas have been yet dealt with by any method of malaria-control. This neglect must have cost millions of lives during the last thirty years.

Malaria-control throughout the Empire is now

everywhere largely in the hands of various committees, which often seem to be elected with little reference to past work in this line or even to past scientific achievement ; and the slowness of advance is probably largely due to this fact. There is nothing to prevent any man calling himself an authority in any line, however little experience he may really have of it ; and a committee so composed may be a very dangerous body (p. 122).

My experiences connected with malaria have not always raised my opinion of *Homo insipiens*, much less of many of those who study, or at least write on that subject ; and my advice to those who propose to enter such an arena is to think twice before doing so.

Sir William MacGregor retired from the Governorship of Queensland in 1914, but died in July 1919. Our great Lord Lister died on the 10th February 1912. Dr. Manson was made K.C.M.G. in 1903 and G.C.M.G. in 1912, and died on the 9th April 1922. General W. C. Gorgas, of the American Army, died at Millbank Hospital in London, on the 4th July 1920, and had been made a K.C.M.G. shortly before his death.

CHAPTER XX

STUDIES ON PATHOMETRY

BY *Pathometry* I mean the *quantitative* study of a disease, either in the individual or in the community. Medical works generally deal only with the *qualitative* study of disease and are usually content with what I call the *sub-science* of the matter, that is with distinctions between names, symptoms, causes, and results. We do not enter upon true science until we come to *measurements*, and enquire, not only *how?* but *how-much?*

My first quantitative studies were, not exactly on pathometry but on an ancillary theme, the random-scattering or diffusion of any living creatures from a centre. If a large number of animals wander or move at random from any one given spot, such as a large number of mosquitoes, for instance, from a single breeding-pool, and are allowed to roam freely in all directions equally until they die, how far from that centre will their bodies be found? My paper [80] was written in 1904 and attempted only an approximate solution; but I asked Professor Karl Pearson to find a better one, as I was only an amateur mathematician. His solution appeared in 1906 and was much more exact [80]. For example, in a circular sterile patch

one mile in diameter in a uniformly breeding country the mosquito density should be reduced to 30 per cent. at the centre, but only to 75 per cent. at the boundary. In a sterile square mile, the density should be 2 per cent. at the centre, 11 per cent. half-way to the boundary, and 50 per cent. at the boundary itself (as I had previously calculated). For further details see [91], section 29. The constants used by Pearson were conjectural numbers supplied by me (see also pp. 125, 166).

Those who accept the mosquito-theory of malaria seem often to think that the presence of even one mosquito of a proper kind is enough to ensure an outbreak of malaria. Then, as a corollary, they argue that as it will always be difficult or impossible to find and to destroy every *Anopheles* anywhere, any attempt to reduce them at all will be a waste of time and money. The facts are, however, that only a proportion of the eggs of an *Anopheles* succeed in hatching out into larvae at all; that only a proportion of these larvae succeed in hatching out into adults; that only a proportion of these adults succeed in biting human beings; that only a proportion of these succeed in biting infected persons; that only a proportion of these succeed in biting persons containing ripe gametocytes; that only a proportion of these live long enough (say one week) to mature the gametocytes in their bodies and to form protospores (p. 57); that only a proportion of these succeed in biting human beings again. The final result must be that only a very small proportion of *Anopheles* in a locality will ever succeed in infecting a new case. *Per contra* the number of *Anopheles* must be large if new cases do

occur—except perhaps—by exceptional bad luck in a few exceptional cases. This is just what has happened in England (and elsewhere) where malaria-bearing species of *Anopheles* still exist but have become so reduced in numbers that the chances are greatly against their carrying the malaria-infection to healthy persons.* Thus from such reasoning alone we derive the very important practical conclusion that in order to counteract malaria anywhere we need not banish *Anopheles* there entirely—we need only to *reduce their numbers* below a certain figure (see also p. 168).

What is that figure? I attempted to find it mathematically in [87] and [91]; but I came only to a provisional conclusion, because I could use only *conjectural constants*, and, much to my disappointment, no one has troubled to obtain better ones since, and I can scarcely do so myself in England. My provisional conclusion was that new cases of malaria will not usually begin to appear unless there are at least about 40 *Anopheles* to each human being during one month; but of course I cannot vouch for the absolute accuracy of that number. It will probably be safer to conclude more generally, without attempting exact figures, as in [91], page 162.

- (1) That the amount of malaria in a locality tends towards a fixed limit determined by the number of malaria-bearing mosquitoes and by other factors.

* An outbreak of malaria actually did occur in England during the war owing to the importation of cases from Salonika, but it soon ceased.

- (2) That if the number of malaria-bearing mosquitoes is below a certain figure, that limit will be zero.

In writing [91] I had time to improve the pathometry used in [87]; and in the second edition of the former book introduced a new chapter ([91], section 66) on what I called the Theory of Happenings, which included (partially) the former work and occupied 35 pages. The problems which I set myself to solve were:

1. Suppose that a population of any organisms lives in some area and that some event happens to a constant proportion of them in unit of time and suppose that all the while the population is affected by a constant birth-rate, death-rate, emigration-rate, and immigration-rate. Then at the end of a given period what proportion of the population will have been affected by the happening once, twice, thrice, etc.?
2. Next suppose that in addition to the data of Problem 1, a constant proportion of these who have been affected by the happening revert to the unaffected class in unit of time, how will the proportion then stand at the end of the given period?

The happening may be almost anything that we can think of—accident, disease, birth, death, marriage, vaccination, receipt of a bequest, conversion to some creed, etc.

I solved both problems at length by the Finite Calculus and gave the solution, but my equations con-

tained many printer's errors, pointed out by Dr. R. Dudfield in 1913.

One of the most important cases is that of what I called Metaxenous Disease—that is, a disease which is common to two species of hosts. This problem led to two simultaneous differential equations, which I could not solve, but which were solved later by Mr. Lotka [102], who developed the subject further.

In [91], p. 676 et seq., I added a conclusion dealing with happenings by the Infinitesimal Calculus also.

Some additions to this work will be found in *Science Progress*, October 1915 and January and April 1916.

It has been known for a long time that statistics of epidemics generally, or frequently, show characteristic “bell-shaped” curves, which, however, tend to decline more slowly than they rise; and in my paper [96] I attempted to show that similar curves might be produced simply by the Theory of Happenings, on the hypothesis that each case of an infectious disease infects so-many new cases in unit of time, while so-many old cases are constantly recovering in the same unit of time, until a large part of the entire population has had the disease, which then dies out. I worked with the Infinitesimal Calculus and solved the fundamental Differential Equation. Next year, the Royal Society enabled Miss Hilda P. Hudson to help me to write the second and third parts of the same paper; but then my health became bad, I found the labour of writing these mathematical papers to be very exhausting, while Miss Hudson—a very expert mathematician—was wanted for war-work. Numerous misprints in the first paper were corrected in the second one.

In [98] I showed that a single dose of a drug which destroys only a proportion of the parasites in a patient must be repeated a number of times before all the parasites can be killed and calculated how many times. But the recent discovery that inoculation of living *Plasmodia* either by mosquitoes or in blood often cures or improves the formerly fatal disease called general paralysis is forcing us to reconsider such calculations.

No sound criticisms of any of these pathometric papers of mine have appeared in this country, so far as I know ; but this is probably because the work is scattered through several publications, namely, in 87, 91 (2nd edition), 92, 96, and 98 ; and I should like to collect and re-write the work in a single small volume, but doubt whether I shall ever be able to do so.

While this book was going through the press, I heard of a paper by W. O. Kermack and A. G. McKendrick (p. 88) on the Mathematical Theory of Epidemics (Proc. Roy. Soc. A., Vol. 115, 1927, pp. 701-721). The paper deals with much the same problem as the paper by me, and by Miss Hudson and by me [96], but uses integral equations. We have not yet had time to consider it carefully, but hope to review it in Science Progress in an early issue in 1929.

CHAPTER XXI

A SUMMARY OF FACTS ABOUT MALARIA*

MALARIAL Fever is perhaps the most important of diseases from the economic point of view, because it haunts almost all warm countries, especially the most fertile rural areas, where it affects millions of people, including children, travellers, planters, traders, officials, and troops ; causes probably one or two million deaths every year besides an immense amount of persistent sickness, and is almost constantly present at certain seasons of every year. Efforts to control it are therefore of the utmost importance to the human race.

The disease was well known to the ancient Greeks and Romans, who rightly connected it more or less with marshy or stagnant water. About 1640, cinchona bark (from which quinine is now made) was introduced into Europe, and was found to cure, or at least to mitigate, the disease if properly given. In 1880, Dr. A. Laveran discovered that the malady is produced by hundreds of millions of minute animal parasites (not bacteria) which live in the blood and multiply simultaneously, as a rule, just before each attack of fever. A few years later, C. Golgi and other Italian savants

* Reprinted with permission from [105].

worked out the development of these parasites (called *PLASMODIUM*) in the blood, and showed that they belong to three different species which respectively cause the three well-known types of malaria, the quartan, the tertian, and the so-called malignant fever. Many hundreds of books and papers have now been written on these organisms and their effects.

In 1883-4, A. F. A. King, A. Laveran, and R. Koch, suggested that the malarial infection may be carried by mosquitoes, which so often breed in marshy or stagnant water. In 1894, the late Sir Patrick Manson added a very strong argument to this hypothesis. Next year I took up the study of the subject in India, always in close communication with Manson in England. After more than two years' constant failures with the common domestic mosquitoes called *Culicines*, on the 20th August 1897 I succeeded in growing one of the *PLASMODIA* of men experimentally in a mosquito of the genus *ANOPHELES* in Secunderabad, but was interrupted for five months shortly afterwards. Next year I worked out the whole life-cycle of this group of parasites in the case of another *PLASMODIUM*, this time of birds, in *CULEX FATIGANS* mosquitoes in Calcutta, and showed that these mosquitoes when infected from feeding on infected birds will carry the same infection into healthy birds which they subsequently bite—and I actually infected numbers of healthy birds in this way. This work showed by analogy exactly how malaria enters human beings; but I was again interrupted in August 1898, and three months later certain Italian scientists, especially A. Bignami, were able to extend my results (with full knowledge of my

previous researches and technique) to the cases of the human PLASMODIA in Italy, which they showed to be carried by Italian ANOPHELES. In 1899, however, H. E. Annett, E. E. Austen, and myself demonstrated the same thing for Sierra Leone ; and since then the story has been repeated in almost all the warm countries of the globe, often with many refinements, and has been published in some hundreds more of papers—*vide* my PREVENTION OF MALARIA, 1911, and my MEMOIRS, 1923 (both published by J. Murray, 50a, Albemarle Street, London, W.1).

The result has been—so far as we know at present—that the malaria-parasites of men are proved capable of living only in ANOPHELES mosquitoes. They start growing in the thickness of the insect's stomach-wall and there produce spores (young ones) which work their way into the insect's salivary glands. These glands produce the irritating poison which is injected into our skin, and the spores enter the human blood with this poison—so that an ANOPHELES first picks up the PLASMODIA from a human patient, then allows them to grow inside her body for a week, and lastly infects one or more healthy persons by her bite. The story is exactly the same as that of birds' malaria ; and the parasites alternate between man and ANOPHELES mosquito or bird and CULEX mosquito. How this process began it is difficult to say ; but a similar process is very common in many parasites of higher orders living in other "hosts" ; and we have no evidence to show, and no reason for thinking, that malaria is carried in any other way. On the other hand, indeed, malaria has often disappeared entirely

after the carrying mosquitoes have been reduced in or banished from a specific area—a thing which would not have occurred if the malarial germs simply rise from the soil, as was previously thought.

A sceptic may now ask, “How can we be so sure that malaria is carried only by *ANOPHELES* mosquitoes?” Very simply, by feeding numbers of mosquitoes of different kinds on a suitable case of malaria and then by dissecting and examining each insect after some days, when the characteristic malaria germs as originally described by me in 1898 will be found only in the *ANOPHELES*. This has been done repeatedly and in many countries during the last thirty years, always with the same result. Another, but a much more laborious way, is to show that new malaria infections occur only in places and seasons where and when *ANOPHELES* are present. Yet another argument is that *ANOPHELES* breed in just those natural waters, such as streamlets, marshes, and pools, which have long been recognized as causing malaria, while the common *Culicines* breed copiously in tubs, pots, cisterns, etc., round houses which are not malarious.* Recently it has been found that artificial infection with malaria is often of great benefit to persons suffering from the otherwise incurable and fatal disease called “general paralysis,” and *ANOPHELES* are now being frequently used to convey this curative malaria—and thus to make many additions to our knowledge as well.

In 1895–9, when I worked in India, almost nothing was known about Indian mosquitoes. Now many volumes and scientific papers on mosquitoes have

* See, however, [106].

been published in many languages by many expert entomologists who have described and classified the insects and have studied their habits and habitats minutely. We now know the prevalent species almost everywhere ; how to breed, feed, and keep them ; and how to infect them with malaria or yellow fever (which is carried by a Culicine mosquito) if required. There are now even special experts on these subjects called "malariologists"—unfortunately they are not numerous enough yet, but they will doubtless become so when their value is more generally recognized.

Of course, everyone knows that the larvae and pupae of mosquitoes exist only in water and hatch out in a week or more into the winged adults, which can then live for a month or sometimes much longer. Only the females suck blood from men or animals, in order to nourish their own eggs. Many hundreds of species are already known—in England they are usually called "gnats." Each species generally likes its own particular collections of water to breed in—tubs, pots, broken bottles, gutters, drains, cisterns, wells, rot-holes in trees, rain-water in palms and other plants ; but the ANOPHELES prefer water on the ground—pools, puddles, ditches, hoof-holes in mire, edges of ponds and rivers, marshes, slush under grass, pools on rocks (even those left by tides), rice-fields, beds of rocky streams, drying water-courses, and even eddies in swiftly-running torrents, and sometimes wells and cisterns. The adults also have their favourite feeding places at night and their resting spots during the day—out-houses, byres, old wells, pits, huts, native houses, roofs, eaves, cupboards, the walls and roofs of rooms

in well-built bungalows, and so on. The larvae of the Culicines generally feed on particles at the bottom of vessels or remain suspended from the surface-film of the water by the breathing-tube at their tails with their head hanging downward; the larvae of ANOPHELES generally float flat on the surface like little sticks and feed on particles which they find floating there. The adult Culicines generally have plain wings and sit on walls with their bodies hanging downwards and inclined to the wall; the adult ANOPHELES generally have spotted wings and sit on a wall with the tail pointed downward and outward at an angle away from the wall. These elementary facts should be familiar to all those who have to live in warm countries, because mosquitoes are not only unpleasant vermin but often very dangerous ones as well. To identify and name the many species of mosquitoes, however, as well as to study their exact breeding-places and sometimes even to find them at all, usually require the services of an expert and the use of special books or pamphlets.

The question is often rightly asked, "How far can mosquitoes fly?" As they can fly as fast as a man walks they may be able to fly a hundred miles during their lifetime of a month or more; but as a matter of fact, like other animals, they tend to remain close to where they were hatched or born, as proved by the common observation that they abound most near their breeding- or feeding-places where such places are isolated. The distance to which they actually do travel must depend on several factors, especially the local distribution of suitable breeding- and feeding-

places. Many species seek shelter and sit close when a strong wind blows ; others, it is said, may drift several miles. As a general rule we may be fairly confident that if they abound in or near a given house they are breeding somewhere close by. Anyway, the mosquitoes coming from a fixed pool must tend to become fewer and fewer in areas of the same size taken at greater successive distances from that pool—somewhat as rays of light and sound become fainter by spreading out with greater distance from their origin. The subject was studied mathematically by me in 1904, and subsequently by Professor Karl Pearson. It is said that if they cannot find food near by they may fly several miles to get it ; but in alleged cases of this kind one always doubts whether the near-by ground has been adequately searched for their breeding-places. On the other hand, if they *can* find feeding near by there is no reason why they should go further ; and cases are on record where malaria carried by them abounded close to their breeding-pools but did not occur even a few hundred yards away. Screens of jungle, of closely-planted trees, of low hills, and of intervening houses may keep them off—but do not always do so. Open country, warm weather and gentle winds should be most favourable for their flight. The golden rule is to seek for their larvae first near at hand, and failing this to look further afield.

The seasons at which ANOPHELES mostly breed are warm seasons when there is sufficient surface-water suitable for their larvae. Too much rain tends to scour out their breeding-waters and also to diminish

the adults, so that the beginning and the end of a rainy season are most suitable for them. In dry, cold weather they may apparently vanish almost entirely in places where they swarmed during the wet, warm weather—though even then a few surviving adults may be found hiding in damp places such as cellars and huts in order to start a new generation when the weather becomes favourable again. Of course, artificial collections of water and rivers and streams often complicate the picture ; and as each species of *ANOPHELES* likes its own particular class of breeding-water it is not always easy to lay down fixed statements on such points about them. Everyone knows, however, that the adults tend to bite most readily in hot, still, damp weather ; and we must remember that as they may live for weeks or months their numbers tend to accumulate towards the end of the mosquito season, that is, generally near the end of the warm rains. This is also generally the season when the largest number of new malaria-infections occur.

Not all species even of the genus *ANOPHELES*—and there are many—are able to carry human malaria. The point has been much studied during the last quarter of a century by three methods. The insects may be fed in captivity on suitable cases of malaria and be then examined microscopically for the parasites ; or they may first be caught in the houses or bedrooms of cases of malaria ; or it may be shown by laborious investigations that a given species of *ANOPHELES* has been associated with a given outbreak of malaria. All three methods are often employed simultaneously,

and we now know that the ANOPHELES may be divided into three groups with regard to their malaria-bearing capacity: a given species (1) may not carry malaria at all, (2) may carry in the laboratory and not in nature, and (3) may carry in both. Those of the third group are evidently the most important, but years of local observation are required to fix the blame adequately on a species. Even different strains of the same species seem to differ as to their malaria-bearing capacity; and the subject is continuing to receive close scientific attention.

Another question often asked is, "How many ANOPHELES of known malaria-bearing capacity are required to produce an outbreak—few or many?" This point also has been studied mathematically by me and subsequently by Dr. Alfred J. Lotka in America. We have to remember the following facts. Only a few eggs of ANOPHELES succeed in hatching-out into adults; only a proportion of these adults ever succeed in biting men; again, only a proportion of these succeed in biting suitably infected men; only a proportion of these become infected from the men; only a proportion of these live long enough (a week or more) to incubate the malaria-parasites fully in their bodies; only a proportion of these succeed in biting another man; only a proportion of the men so bitten become infected. The total result must be that only a very small proportion of the ANOPHELES hatched out in a locality can ever succeed in infecting a new case. In other words, new cases will not occur in a locality unless the number of ANOPHELES of definite malaria-bearing capacity in that locality is very large—except,

perhaps, by some very exceptional bad luck in a few very exceptional cases. From such close reasoning (which can be easily put into mathematical form) we derive the very important practical conclusion that in order to control malaria we need not banish every ANOPHELES from a locality—we need only to reduce their numbers below a certain figure *per* man. This is just what has happened in England (and elsewhere) where malaria-bearing ANOPHELES still exist but have become so reduced in numbers that the chances are greatly against their carrying the malaria-infection to new cases.* What the limiting proportion of ANOPHELES *per* man actually is cannot be calculated at present because all the data are not yet accurately known. The equations contain many variable factors each of which affects the final result, but they cannot be discussed here in detail.

The number of new cases of malaria must depend not only on the number of carrying ANOPHELES but on the number of old cases of malaria from which the ANOPHELES carry the parasites into the new cases, and not only on these but on the accessibility both of the old and the new cases to the insects. Hence we can employ three methods for controlling malaria, namely (1) mosquito-reduction, (2) cure of the old cases of malaria with quinine, and (3) exclusion of mosquitoes by means of nets and screens. Books may be, and have been, written on these three methods. Each is appropriate under certain conditions; two

* An outbreak actually did occur in England during the war, owing to the importation of malaria cases from Salonika; but it soon ceased.

of them may be combined under other conditions ; and sometimes all three may be used together, as when the Panama Canal was being made.

All three methods cost money. But before considering them at all we must remember what is frequently forgotten, what is, in fact, the fundamental economical consideration, that malarial fever itself costs a great deal of money. It cripples thousands or millions of people for months or years ; it often impairs the whole labour-force of a plantation or of a village or town just when the crops require the closest attention ; it fills the hospitals (which are expensive institutions) ; it often demands treatment- and maintenance-allowances ; and it generally doubles (at least) the death-rate in localities in which it abounds. A planter in Ceylon told me that it cost him a thousand pounds a month during the malaria-season on one of his plantations alone. It is often one of the most expensive items in the cost of military campaigns ; and it has even caused the abandonment of whole villages and stations and of extensive areas of cultivation. We must always remember these facts when we talk of the cost of malaria-prevention. Both malaria and malaria-control cost money ; but the former costs health and even life itself in addition.

Local conditions often vary so much, even a few hundred yards apart, as to the breeding-waters of the ANOPHELES, the species concerned, their respective habits and carrying capacities, the accessibility of human habitations, and the best methods of dealing with particular waters, that it is quite impossible to give any brief account of the great anti-malarial

measure of mosquito-control, which is obviously the best measure where practicable. The work required for it is divisible into two classes—temporary and permanent work. In temporary work, breeding-waters are treated with oil, or with Paris Green, or other chemicals, or even by introducing small fish to eat the larvae, or by removing surface-water-weeds or grass which shelter the larvae from fish, or by straightening the banks of small streams, and so on. Permanent works, such as draining marshes, rectifying the banks of lakes, rivers and streams, draining ravines (in which some of the worst *ANOPHELES* live), cutting down marshy jungles, excluding tidal waters, and many others, are generally much more expensive and require the assistance of engineers and often of specially trained engineers. The work can sometimes be done by an energetic manager alone, or with the help of a local medical officer; but in wet or water-logged areas it demands the preliminary surveys of a malariologist, whose advice will often save wastage of funds on dealing with harmless waters. In some cases also legislation may be required to control defaulting neighbours or villages. It is for the future to organize all such work so as to get the best results over a large tract of country for the least expenditure of money and time.

The second method of malaria-control is to treat all the old cases of the disease persistently with quinine until they have no *PLASMODIA* left in them to be carried into the blood of others by the local *ANOPHELES*. This requires repeated examination of the blood of all persons in the locality by a medical man or other expert. It is not enough to judge merely by the symptoms,

because many people, especially native children (who are most frequently infected), may contain swarms of the parasites and yet may show no symptoms at all for considerable periods. Then again, quinine must be given regularly for some time in order to have the desired effect, and many people, especially Indian coolies, object to taking it. The cost of the quinine and of the necessary medical attendance is likely to be great with large numbers of people. Nevertheless, the method has a double advantage—it cures the old cases besides preventing new ones; and it should be used as much as possible if only for the sake of those old cases, which otherwise tend to continue “relapsing” for month after month, even where there are no mosquitoes. At the end of the war, two whole divisions badly infected with malaria at Salonika were put into the front line in France, owing to the able quinine treatment given to them by Col. J. Dalrymple, R.A.M.C. (T.C.)—see *Observations on Malaria* (edited by me), War Office, December, 1919. An extension of the method consists in what is called general quinine prophylaxis—everyone has to take quinine as a preventive whether he is infected or not. It produced only temporary protection in Salonika and is troublesome and expensive, but has been much advocated by French medical men.

The third method of malaria-control, exclusion of mosquitoes by mechanical means, can be employed by everyone if he chooses. ANOPHELES feed almost entirely during the hours of darkness or twilight, and most persons are infected during sleep. The ordinary bed-net is perhaps the best precaution against malaria.

The top as well as the four sides should be made of netting, not less than fourteen holes to the inch. If mosquitoes are numerous I advise that the net should be suspended *inside the poles* and tucked under the mattress ; and in this case the lower part of the sides (not the top) should be made of long-cloth because the hands and feet of the sleeper may come into contact with the net just above the mattress and can then be easily bitten by outside mosquitoes through ordinary netting. The cost of a bed-net is trifling compared with that of a malarial infection. I myself have been infected with malaria only once in spite of sixteen years' service in India and thirteen subsequent " malaria-expeditions " to warm climates ; and I attribute this good fortune chiefly to my scrupulous use of the bed-net. If the net is drawn tight and the bed is close to a window there is very little impediment to the entry of air. Punkas, or fans inside or outside the nets, or by themselves, are now being largely employed. Without such precautions a person may be bitten by scores of mosquitoes during a single night, and if some of these are ANOPHELES, he may be infected with malaria within a day or two of arrival in the tropics. It is astonishing how careless people are even when they possess nets—which may be left full of holes or hitched up anyhow against the bedding. Well-to-do Indians now use nets much more generally, but often without proper hanging ; and there is no reason why they should not be used by the poorest for themselves and for their families. It would be a " profitable charity " for planters' managers to make their coolies employ them. The Indian generally

sleeps with a corner of his sheet over his head, but is probably bitten much nevertheless ; and his children frequently become infected with malaria shortly after birth. Personally, I advise the use of a bed-net everywhere and for everyone in the tropics, at all seasons, and whether mosquitoes are to be seen or not.

Wire-gauze protection to houses, or at least to bedrooms, is now frequent in India, and should always be encouraged. Where a number of people sleep in the same room, as in hospitals, asylums, schools, work-houses, and in some workmen's dormitories or coolies' lines, it is absolutely imperative and should be made compulsory by law. In such cases, a single infected *ANOPHELES* may infect person after person within a few nights by wandering from one to another, because its salivary (or poison) gland may still contain hundreds of the malaria-spores after it has bitten several people. Under these circumstances the entry of an infected *ANOPHELES* is about as dangerous to the inmates as the entry of a tiger would be ; and such a mosquito, finding herself in the presence of plenty of food, is likely to haunt her comfortable quarters for weeks or months, infecting or re-infecting everyone in the ward or dormitory. The sudden and widespread outbreaks of malaria among coolies are often to be explained by this "congregate sleeping" ; and yet little attention is paid to the point, even now after a quarter of a century. The same danger exists with all families who sleep unprotected in the same room, and especially with Indian coolies and their wives and children who generally occupy the same little chamber indefinitely—the mud walls and bare bamboo-

roofs often harbour numbers of mosquitoes for months, and there is no difficulty in seeing why such people are so "full of malaria." Possibly the same old female ANOPHELES become regular "man-eaters," like some tigers, while their relations live only on cattle or birds in the open. The same thing was found to occur in camps in Panama when I was there in 1904, and has doubtless been responsible for much of the malaria among troops on active service, and their followers, from time immemorial. In a hospital ward at Wilberforce, Sierra Leone, in 1899, we actually found malaria-parasites in a quarter of the patients present and at the same time in a quarter of the ANOPHELES caught in the ward! Unscreened hospitals are very often centres for malaria-infection. Dark go-downs and huts with smoke-begrimed walls and open rafters and roofs are frequently full of ANOPHELES which descend and bite the inmates every night. Much greater care should be taken, and is being taken, with the housing of officials, servants, coolies, and labour forces. Windows should be large, roofs ceiled with sheets of corrugated iron, and roofs and walls should be kept well painted white, or at least frequently whitewashed, so as to avoid the dark corners which mosquitoes love. The expense of building proper "cooly-lines" must be small compared with that of annual outbreaks of malaria (and other diseases); and the wretched native shanties which one still sees all over India, and even in the heart of Calcutta, ought to be suppressed by law. People often say that the Indians prefer to live in them, but I doubt it—too often they have no experience of anything better.

176 SUMMARY OF FACTS ABOUT MALARIA

The lay reader will wish to know how we estimate the amount of malaria in a locality to justify us in saying that the disease is increasing or diminishing. The ideal way is for an expert to examine every person living within the area, excluding new-comers, for the symptoms of malaria, their history, their blood, their spleens, and their appearance. Where there is no time for this, "random-samples" of the population are taken, the results being corrected for "statistical error." A rapid and yet very accurate measurement is derived merely from the "spleen-rates" of the children under twelve or fifteen years of age. In all malarious countries their spleens are frequently enlarged; and the enlargement can be felt or even seen so quickly that for a rough but useful estimate 100 children may be examined within an hour; and the spleen-rate is the ratio of the children with enlarged spleens to every hundred of them examined. The rate varies from 100 per cent. in very malarious places to zero in non-malarious ones; but it may be about 5 per cent. or more in healthy areas close to unhealthy ones, owing to importation of cases from outside; and it is usually from 30 per cent. to 60 per cent. in moderately malarious localities. The general death-rate of a country is a useful indication, because it is often double or more in a malarious colony or city to what it is in a non-malarious one otherwise in the same condition—witness Mauritius and the Seychelles, for example. The general death-rate also shows a great rise during the malaria season, which is not to be found in the statistics of non-malarious countries. The malaria-admissions at hospitals constitute another test if other

causes of variation in the figures are excluded. But, of course, all these methods of measurement require more or less expert knowledge. For a planter, perhaps the most reliable test of all is his annual loss of labour during the malaria-season, not to mention its effects on his servants, his family, and himself !

The actual treatment of cases must be left to medical men ; but the lay reader should understand that so long as patients continue to harbour malaria-parasites at all they will be subject to relapses of fever, and may also infect other people if any of the carrying ANOPHELES can succeed in biting them.

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INDEX

- Aberdeen Quater-centenary, 133
 Académie de médecine, Paris, 41
 Accra, 97
 Acknowledgements by Grassi, 25
 Advisory Committee, Central Industrial, Anti-malarial, 151
 Africa, British Central, 67
 third visit, 105
 Agramonte, Aristides, 71
 Almquist, E., 105
 Anderson's College, Glasgow, 125
 Annett, H. E., 42, 47, 68, 83
Anopheles (see *Mosquitoes*)
 Assam, 16, 43, 151
 Athens, 129-142
 Austen, E. E., 42, 60
 Awards to inventors, Royal Commission, 149
 Aziz, Mehmed, 142

 Bagshawe, A. C., 141
 Balch, L., 128
 Balfour, Andrew, 126
 Balfour, Lord, 148
 Bangalore, 5, 8
 Barbadoes, 44
 Bastianelli, G., 20, 33, 40
 Bentinck, Lord Cavendish, 150
 Berkeley, W. H., 89
 Bibliographies, alleged, 36
 Bignami, A., 6, 7, 8, 11, 21, 26, 27, 30, 31, 32, 33, 40
 Birds' malaria, 15, 17, 32
 Bland-Sutton, J., 30
 Bombay, 138
 Boyce, Rubert W., 41, 83, 114, 124, 132, 140, 141
 Boyle, Sir Cavendish, 135
 Bridger, J. F. E., 150

 British Association, 104
 British Medical Association, 16, 21, 30, 103, 125
 Broughton-Alcock, W., 145
 Brussels Medical Congress, 125
 Buchanan, A., 66

 Cairo, 117
 Calandruccio, S., 20, 37, 39
 Calcutta, 14, 15, 17, 18, 26, 75, 151
 Carroll, James, 71, 72
 Carter, H. R., 71, 72, 128, 150
 Carter, Vandyke, 3, 10
 Carter, W., 83
 Castellani, Aldo, 150
 Celli, A., 63, 64
 Ceylon Tea Association, 150
 Chamberlain, Austen, 148
 Chamberlain, The Rt. Hon. Joseph, 60, 81, 88, 123, 124, 129
 deputation to, 82-85
 Charles, T. Edmonston, 26, 27, 31, 34
 Charmoy, D'Emmerez de, 135
 Christophers, S. R., 67, 68, 78
 Chromatin, 8
 Cleveland, A., 142
 Clue, 14
 Coats, James, jnr., 85, 86, 87, 88, 105, 112, 124
 Colon, 127
 Colonial Office, 41
 Commission, proposed, 84, 85
 Conclusions, Grassi's, 23
 Congregate-sleeping, 174
 Conjectures *versus* proofs, 71
 Conjoint paper, 33

- Cooly Lines, 175
 Councilman, J., 127
 Crescents, 4, 5, 7, 14
 Crewe, Lord, 139
 Crichton-Browne, J., 61
 Crucial-experiments, 64-66
Culex-gang, 90, 91
 Culicifuges, 79
 Curepipe, 135
 Cyclone, 128
 Cyprus, 142
 Cytology, 54, 58
- Dalrymple, J., 146
 Daniels, C. W., 17, 43, 67, 68, 98, 99, 100
 Dardanelles, 144
 d'Arenberg, Prince Auguste, 114
 Denton, Sir George, 89, 102
 Derby, The Earl of, 124, 129
 Dieppe, 146
 Discovery, the Fundamental, 9
 Doty, A. H., 89, 128
 Drainage and mosquito-control, 111, 112
 Drake-Brockman, R. E., 145
 Durning-Lawrence, Lady, v
 Durning-Lawrence, Sir Edwin, v, 141
 Dutton, J. Everett, 102
- Elder Dempster & Co., 42
 England, leave to, 1899, 18
 Entomology, Indian mosquitoes, 30
 Enumerative studies, 141
 Epidemics, pathometry of, 154-159
 Errors, popular regarding mosquito-control, 100, 101
 Exclusion, mosquito-, 172-175
 Experimental infection, 66, 67
 Exploitation, 113
 Eyre, J. J., 63
- Fearnside, F. C., 66
 Fielding-Ould, Robert, 52, 54, 60, 83
 Finlay, Charles, 70, 71, 72
 Forde, R. M., 102
- Foster, Michael, 78
 Fourth stage, 47-55
 Fowler, C. E. P., 134
 Freetown, 41-46, 67, 75, 89, 90, 97, 98, 102, 105, 106
 Wilberforce Barracks, 44, 97
 lecture at, 92
 insanitary condition of, 106, 107
 Frequency, detection by, 22
- Galli-Valerio, Dr., 105
 Gambia, British, 89, 102
 George, Lloyd, 148
 Germinal threads, 32
 Giles, G. M., 29, 43, 60, 62, 63
 Go-downs, 175
 Gold Coast, 102, 103
 Golgi, C., 2
 Goold-Adams, Sir Hamilton, 142
 Gorgas, W. C., 74, 75, 86, 100, 122, 126, 127, 153
 Grassi, G. B., 6, 8, 20-36, 38, 39, 40, 42, 45, 46, 60, 62, 124
 Grassi's results, 24
 Greece, 129, 132, 142, 143
 Greek, Anti-Malaria League, 132
- Haffkine, W. H., 15, 121
 Harrison, Meredith, 146
 Hartman, E., 18
 Harvey, Surgeon-General, 88, 89
 Havana, 126, 127
 Health, effect on, of mosquito-control, 109, 110
 Hedging, 24
 Hehir, Patrick, 10
 Herdman, William, 56
 Hidden hand, 119
 Hodgins, Captain, 89
 Holland, malaria in, 89, 119
 Holt, John, 88
 Hong-Kong, 126
 Hospitals, 175
 for Indian troops, 144
 special malaria, 146
 House-control of mosquitoes, 75, 79
 Hudson, Hilda P., 158
 Huts, 175

- Ibadan, 96
 Ilissos, 132
 Imperial Sanitary Commission, 125
 Incredulity, 121
 India, 137-139
 Indian mosquitoes, 4, 30
 Infection, experimental, 16
 Ingenious devices, 78, 79
 Injured innocence, 122, 123
 Interruption, 13
 Investigation, first stage of, 4
 Ismailia, 113-119
 Italy, 6, 7, 19-40
- James, S. P., 146
 Jamieson, D. H., 145
 Jansc6, 66
 Jaureg, J. Wagner-, 67
 Jenner Institute, 113, 114
 Jones, Alfred, 41, 45, 82, 88, 89, 104, 114, 124, 132, 140, 141
 Jones, W. H. S., 132
- Kala-azar, 16, 17
 Kardamatis, J. P., 129
 Keogh, Alfred, 134, 144
 Khartoum, 126
 Kherwara, 9, 13, 14
 Kilborne, F. L., 8, 21
 King, A. F. A., 2, 8
 King, W. G., 76, 121, 137, 138
 King-Harman, Sir Charles, 89, 106
 Kissinger, J. R., 72
 Klang, 126
 Koch, Robert, 20, 26, 34, 36, 62, 68, 69, 104
 Kopais, Company of, 129
 Kopais, Lake, 129, 143
- Labbé, A., 18
 Lagos, 52, 92, 96
 Lankester, E. Ray, 61, 62
 Laveran, Alphonse, 2, 5, 10, 20, 34, 36, 41, 42, 61, 104
 discovers *Plasmodia*, 2
 Lawrence, W. F., 82
 Lazear, J. W., 71, 72
 Lectures, public, 119
- Leopold II, H. M., 132, 133
 Le Prince, J. A., 128
 Leverhulme, Lord, 140
 Linares mines, 142
 Lister, Lord, 36, 38, 42, 47, 67, 104, 113, 114, 122, 153
 memorial to, 67
 letter to, from Ronald Ross, 48-54
 Liverpool Chamber of Commerce, 58, 103, 104
 Liverpool University College, 41
 Liverpool School of Tropical Medicine, 10, 42
 London School of Tropical Medicine, 10, 147
 Lorans, Dr., 135
 Lotka, A. J., 158, 168
 Low, G. C., 66
 Lyster, T. C., 72, 128
 Lytton, Lord, 151
- Maccarese, 32
 MacCallum, W. G., 5, 8, 14
 Macgregor, Malcolm E., 136
 MacGregor, Sir William, 86, 87, 92-96, 113, 114, 116, 117, 119, 125, 129, 153
 McKendrick, A. G., 88, 97, 159
 MacLeod, Sir Charles, Bt., 150
 Malaria, Anti-, Greek League, 132
 Malaria-control, methods of, 170
 Malaria, entry of, 131-132
 theories of, 1-3
 Malaya, 151
 Mannaberg, J., 11, 104
 Manson, P., 5, 7, 8, 9, 11, 12, 13, 14, 15, 16, 17, 21, 25, 26, 27, 30, 31, 35, 36, 38, 41, 59, 64-66, 99, 126, 153
 Goulstonian Lectures, 6
 hypothesis, 3
 Manson, P. Thorburn, 65, 66
 Marchiafava, E., 2, 11, 63
 Marshall, E., 145
 Marshall, G. A. K., 8
 Mauritius, 131, 134, 135, 136
 Medical Society, 135

- Megaw, J. W. D., 151
 Metaxeny, in Protozoa, 17, 20, 158
 Mian-Mir, 137, 138, 139
 Microphotographs, 56
 Milne, A. H., 114
 Misapprehensions, 99
 Misrepresentations, 18
 Mohamed Bux, 75
 Moran, J. P., 73
 Mosquitoes :
 Aedes, 5, 12, 34, 43, 44, 72, 73, 115, 116, 117
 Anopheles, attitude of, 60
 elementary differences in, 164, 165
 and fish, 49
 gang, 90, 91
 pools, 42, 90, 130
 Culex, 5, 12, 15, 34, 43, 44, 72, 73, 115, 116, 117
 attractors, 79
 brindled, 11
 classification of, 11
 dappled-winged, 12, 27, 28, 30, 42, 76
 Day, 13
 dissection of, 5, 11, 17
 eggs of, 8, 28
 exclusion, 65, 93
 extirpation of, 52, 77
 extirpation of, versus reduction, 155, 168
 feeding of, 4, 12
 flight of, 165, 166
 grey, 11, 26
 Indian, 4, 30
 lamp, 79
 larvæ, 4, 31, 165
 parasites in, 9
 pathogenic, 167-77
 random-migration of, 125, 154, 166
 reduction of, 170, 177
 repellants, 79
 seasons, 166-167
 species of
 Aedes aegypti, 71
 A. culicifacies, 9, 13, 29
 A. costalis, 43, 44, 45, 67, 136
 Mosquitoes :—continued.
 species of :—continued.
 A. claviger, 24, 27, 28, 30, 33
 A. funestus, 43, 60, 67, 136
 A. listoni, 43
 A. malariferi, 37, 40
 A. maculipennis, 33
 A. pharoensis, 116
 A. rossii, 17, 43
 A. stevensi, 9, 29
 trap, 96
 with spotted wings, 9
 Mosquito-control, 74, 77, 78, 80, 81, 107, 108, 118, 152, 153
 general, 90, 91
 special, 90
 cost of, 109
 and drainage, 111, 112
 Moulki, 130, 143
 Murray, C. H., 10
 Murray, Sir John, 143
 Muspratt, Max, 88
 Musser, Dr., 127

 Nathan, Sir Matthew, 97
 Nature, 38
 Newcastle Clinical Society, 125
 Nobel Prize, 34, 37, 119
 Nomenclature, 53, 56-58
 Northumberland, The Duke of, 61
 Nuttall, G. H. F., 58, 59, 61

 Object-lesson, 88
 O'Connor, F. W., 145
 Ootacamund, 8, 22
 Opie, 14
 Orkhomenos, 131
 Osler, William, 127, 131
 Oxford Medical Society, 132

 Palm-leaf fan, 93
 Panama, 127
 Papers versus work, 81
 Pares, Bernard, 140
 Pathometry, 136, 154-159
 Patrick, Dr., 142

- Pearson, Karl, 126
- Pigment, 9, 68
- Pigmented cells, 14, 15, 32, 35, 43, 44, 45
- Piroplasmosis, 8
- Plasmodia*, 6, 19, 36
 - in *Anopheles*, 162, 163
 - flagellate-bodies, 5, 6, 7, 8, 12, 32, 33
 - position of, 12
- P. Relicta*, 15, 16, 17, 18, 19, 40, 64
- P. falciparum*, 17, 19
- Plimmer, H. G., 3
- Popular books, 59
- Portland, Her Grace The Duchess of, 150
- Port Louis, 135
- Port Said, 115
- Port Swettenham, 126
- Possibility, an alternative, 27
- Prevention by treatment, 69, 171-173
- Price, G. B., 145
- Princess Christian, H.R.H. The, 129
- Priority, 17
- Proofs, 66
- Proteosoma*, 18, 19, 26, 36
- Pressat, A., 116, 118
- Puits perdus*, 117

- Quinine treatment and prophylaxis, 11, 69, 116, 146, 159, 171, 172

- Reed, Walter, 71, 73
- Rees, Dr., 8
- References, 179-189
- Research, 36-37
- Robertson, J. C., 145
- Rome, 26, 36
 - letters from, 26
- Ross, H. C., 151
- Ross Institute, 150
- Ross, J. W., 128
- Ross, Ronald :
 - appointments, 144-145
 - birth, 1
 - Cameron Prize, 114
 - Honours, title-page
 - Memoria del Ross, 61
 - my paper of 1897, 28-30
 - Nobel Prize, 119, 120
 - parliamentary petition, 147-149
 - professorship, 119
 - resignation of Committee, 141
 - resignation of Professorship, 141
 - Ross-cycle, 63
 - Ross Institute, 150
 - Russia, visit to, 140
 - schooling, 1
 - service, 1
 - studies, 1
 - work on malaria commenced, 1
- Royal College of Surgeons, 89
- Royal Institution, 61
- Royal Society, 17, 38, 42, 67, 69, 89, 122, 141

- St. Louis, Congress, 1904, 127
- Sakharoff, N., 8
- Salonika, 144, 145
- Sambon, L., 66
- Sanitary Commissioners, 83, 124
- Sanitary education, 126
- Savas, S., 129
- Sceptics, 106
- Schaudinn, F., 57
- Schüffner, W., 66
- Science Progress, 143
- Scientists, various, 80
- Second stage of investigation, 14
- Secunderabad, 3, 4, 5, 9, 13
- Seychelles, 134
- Shakespeare, W., 150
- Sherrington, Sir Charles, 114
- Shipley, A. E., 38, 61
- Sigur Ghat, 9
- Simla, 138
- Simpson, G. E. C., 141
- Simpson, W., 150
- Smith, T., 8, 21
- Smyth, J., 30
- Sola, 31, 32

- Splenic enlargement in children, 68, 69
 Spleen-rate, 130
 Steele, B., 129
 Stephens, J. W. W., 67, 68, 78, 145, 147
 Sternberg, Surgeon-General, 71
 Stott, W., 126
 Strachan, H. W., 92, 94, 96 and Lagos, 52
 Strickland, C., 151
 Suez, 117
 Summary, 160-177
 Swanzy, F., 82, 83, 88, 103
 Sydenham, Lord, 138
 Sydenham, Society, New, 11

 Taranto, 145
 Taylor, Logan, 87, 90, 98, 103, 105, 106, 109, 110, 112, 123
 Temerarious proposal, 85
 Terzi, M., 66
 Thebes, 131
 Theobald, F. V., 62
 Theory of Happenings, 157, 158
 Thin, Dr., 30
 Third stage, Italy, 19, 40
 Third stage, Freetown, 41-46
 Thomson, David, 141, 144
 Thomson, J. C., 86, 141
 Todd, J. L., 132

 Tropical Sanitation Fund, 87, 102, 105, 112, 123
 Tropical Medicine, Liverpool School of, 10, 61, 81
 Tropical Medicine, London School of, 10, 147
 Tropical Medicine, Society of, 141

 Vacoas, 135
 Vacuum-theorists, 125, 137
 Van Neck, Dr., 54
 Venezelos, M., 143
 Versions, two, 23
 Victory, a dash for, 74

 War Office investigations, 146
 War Office, medical, 145
 Warren, Mr., 66
 Watson, Malcolm, 150, 151
 Webb, Sir A. L. A., 145
 Weeks, H. Claye, 127
 Weyman, Surgeon-General Walter, 127
 Williamson, G., 88
 Windows, mosquito-proof, 175
 Woodhead, G. Sims, 61

 Yates, Edith, 143
 Yellow fever, 70-73, 75

 Zoological bearings, 56

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